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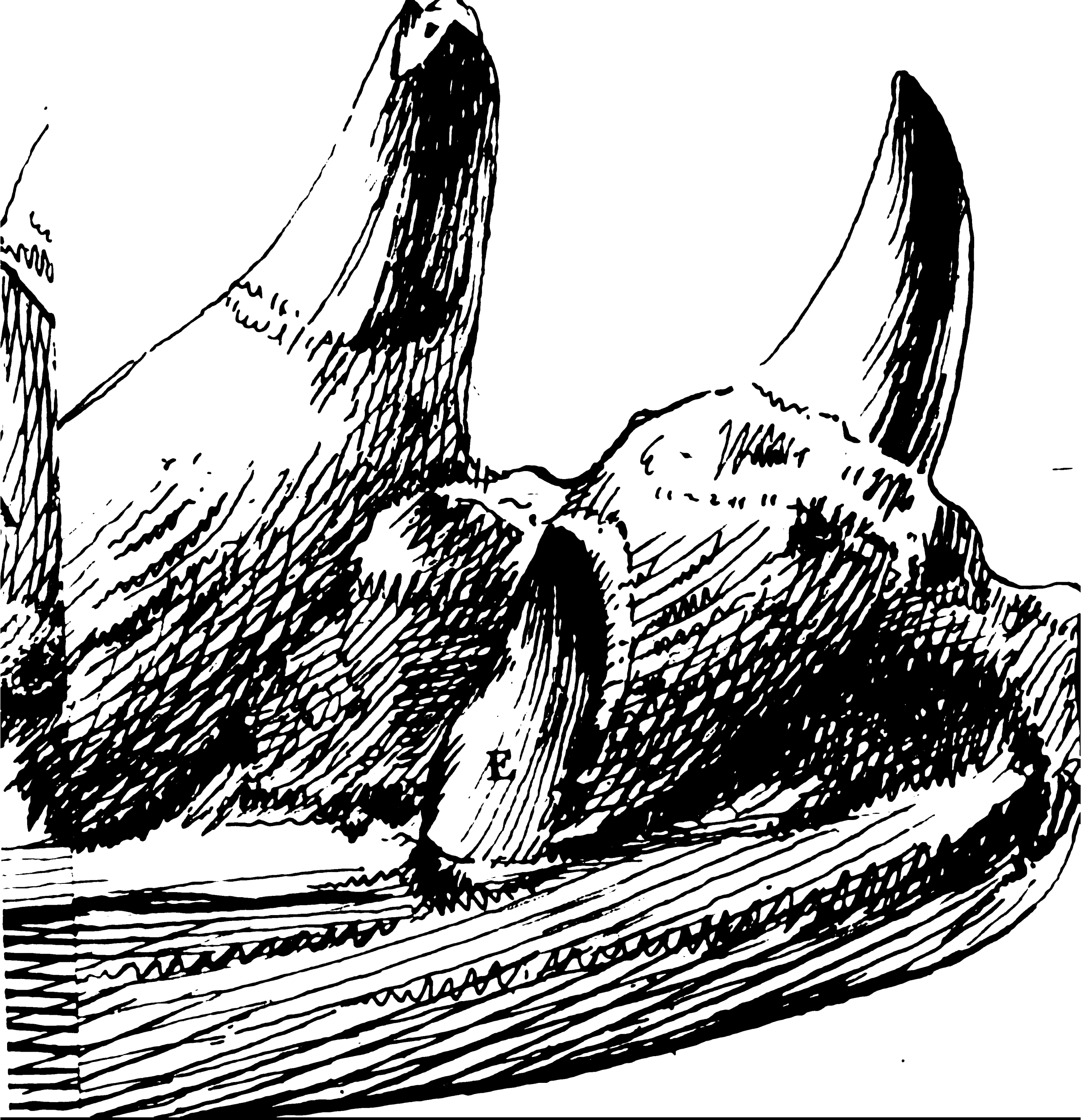

Philosophical Transactions

Royal Society (Great Britain), Royal Society (Great Britain).

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PHILOSOPHICAL TRANSACTIONS,

OF THE

ROYAL SOCIETY

OF

LONDON.

VOL. LXXVI. For the Year 1786.

PART I.

BIBLIOTHECA BODLEIANA

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THE Committee appointed by the *Royal Society* to direct the publication of the *Philoſophical Tranſactions*, take this opportunity to acquaint the Public, that it fully appears, as well from the council-books and journals of the Society, as from repeated declarations which have been made in ſeveral former *Tranſactions*, that the printing of them was always, from time to time, the ſingle act of the reſpective Secretaries, till the Forty-ſeventh Volume: the Society, as a Body, never intereſting themſelves any further in their publication, than by occaſionally recommending the revival of them to ſome of their Secretaries, when, from the particular circumſtances of their affairs, the *Tranſactions* had happened for any length of time to be intermitted. And this ſeems principally to have been done with a view to ſatisfy the Public, that their uſual meetings were then continued for the improvement of knowledge, and benefit of mankind, the great ends of their firſt inſtitution by the Royal Charters, and which they have ever ſince ſteadily purſued.

But the Society being of late years greatly inlarged, and their communications more numerous, it was thought adviſable, that a Committee of their members ſhould be appointed to reconſider the papers read before them, and ſelect out of them ſuch, as they ſhould judge moſt proper for publication in the future *Tranſactions*; which was accordingly done upon the 26th of March 1752. And the grounds of their choice are, and will continue to be, the importance and ſingularity of the ſubjects, or the advantageous manner of treating them; without pretending to anſwer for the certainty of the facts, or propriety of the reaſonings, contained in the ſeveral papers ſo publiſhed, which muſt ſtill reſt on the credit or judgment of their reſpective authors.

It

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It is likewiſe neceſſary on this occaſion to remark, that it is an eſtabliſhed rule of the Society, to which they will always adhere, never to give their opinion, as a Body, upon any ſubject, either of Nature or Art, that comes before them. And therefore the thanks, which are frequently propoſed from the chair, to be given to the authors of ſuch papers as are read at their accuſtomed meetings, or to the perſons through whoſe hands they receive them, are to be conſidered in no other light than as a matter of civility, in return for the reſpect ſhewn to the Society by thoſe communications. The like alſo is to be ſaid with regard to the ſeveral projects, inventions, and curioſities of various kinds, which are often exhibited to the Society; the authors whereof, or thoſe who exhibit them, frequently take the liberty to report, and even to certify in the public news-papers, that they have met with the higheſt applauſe and approbation. And therefore it is hoped, that no regard will hereafter be paid to ſuch reports, and public notices; which in ſome inſtances have been too lightly credited, to the diſhonour of the Society.

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THE Preſident and Council of the Royal Society adjudged, for the Year 1785, the Medal on Sir GODFREY COPLEY's Donation, to Major-General WILLIAM ROY, for his Meaſurement of a Baſe on Hounſlow-Heath.

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PHILOSOPHICAL TRANSACTIONS.

I. *Obſervations on the Graduation of Aſtronomical Inſtruments; with an Explanation of the Method invented by the late Mr.* Henry Hindley, *of* York, *Clock-maker, to divide Circles into any given Number of Parts. By Mr.* John Smeaton, *F. R. S.; communicated by* Henry Cavendiſh, *Eſq. F. R. S. and S. A.*

Read November 17, 1785.

PERHAPS no part of the ſcience of Mechanics has been cultivated by the ingenious with more aſſiduity, or more deſervedly ſo, than the art of dividing Circles for the purpoſe of Aſtronomy and Navigation. It is ſaid, that TYCHO BRAHE

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and HEVELIUS laboured this part of their inſtruments with their own hands; and though public rewards have at length brought forth different methods of dividing from our beſt artiſts, which have been communicated to the Public; yet I truſt it will be thought, that if any thing relative to this buſineſs remains yet behind, that may tend to furniſh the ingenious artiſts, who are cultivating this field, with any new or curious idea upon the ſubject, it will be well worth communicating to this learned Society: ſince, if an hint, which is eſſentially different from any thing that (ſo far as I know) the Public is in poſſeſſion of, be once ſtarted, and is purſued and worked upon by ingenious men, it is not poſſible to ſay, to what valuable purpoſes it may be converted.

This, perhaps, will better appear by taking a ſhort review of the labours of others, from the time of TYCHO BRAHE and HEVELIUS (who did not uſe teleſcopic ſights) to the preſent time.

The very learned, ingenious, and inventive Dr. HOOK, in his Animadverſions on the *Machina Cæleſtis* of HEVELIUS, publiſhed in the year 1674, has given us an elaborate deſcription of a quadrant, whoſe diviſions were formed, and afterwards read off, by means of an endleſs ſcrew, working upon the outermoſt border of the limb of a quadrant; which, he ſays, *does not at all depend upon the care and diligence of the inſtrument-maker in dividing, graving, or numbering the diviſions, for the ſame ſcrew makes it from end to end*; yet he has given us no account of any particular care or caution that he uſed, in preventing the ſame ſcrew from making larger or ſmaller paces, in conſequence of unequal reſiſtance, from a different hardneſs of the metal in different parts of the limb; nor any method of correcting or checking the ſame; nor of making a ſcrew, the angle of whoſe threads with the axis ſhall be equal in every part of the circumference; therefore the whole of this buſineſs (in which accurate *mechaniſts* well know conſiſts the whole of the difficulty) he refers to the *ingenious workman*; and, in particular, to the then celebrated Mr. TOMPION, whom, he ſays, he employed to make his inſtrument, and who had thereby *ſeen and experienced the difficulties that do occur therein:* but was any ingenious workman now to purſue the directions of Dr. HOOK, ſo far as his communication thereof extends, we may conclude, that he would make a very inaccurate piece of work, far inferior in performance to what the Doctor ſeems to expect from it *. But yet, I believe, it was the *firſt* attempt to apply the endleſs ſcrew and wheel, or arch, to the purpoſe of forming diviſions for aſtronomical inſtruments; for, the Doctor ſays himſelf, the perfection of this inſtrument is the *way* of making the diviſions; that it *excels all the common ways of diviſion:* and in the table of contents it is intituled, *An Explication of the new Way of dividing.*

This method, however, of Dr. HOOK's was not laid aſide without a very full and ſufficient trial: for Mr. FLAMSTEED, in the *Prolegomena* of the third volume of *Hiſtoria Cœleſtis*, informs us, that *he* contrived the ſextant, wherewith his obſervations were chiefly made, from his entrance into the Royal Obſervatory in the year 1676 to the year 1689. This ſextant was firſt made of wood, and afterwards of iron, with a braſs limb of two inches broad, by Mr. TOMPION, at the expence of Sir JONAS MOORE; the radius thereof was 6 feet 9½ inches; it was furniſhed with an endleſs ſcrew upon its limb of 17 threads in an inch, and with teleſcopic ſights*. Of this inſtrument Mr. FLAMSTEED gives the figure at the latter end of his *Prolegomena* before-mentioned, ſufficiently large to ſee the general deſign; the whole being mounted upon a ſtrong *polar axis* of iron, of three inches diameter.

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* This was indeed verified in an attempt upon the ſame plan by the Duc DE CHAULNES, publiſhed in a Memoir of the Royal Academy of Sciences at Paris, for the year 1765.

Though, in the full deſcription of this inſtrument, Mr. FLAMSTEED mentioned the Limb's being furniſhed with *diagonal diviſions*, diſtinguiſhing the arch to 10 ſeconds†; yet it is pretty clear, that it had not theſe originally upon it; but that the dependance was wholly upon the ſcrew diviſions, when it came out of Mr. TOMPION's hands. This one may reaſonably infer from the obſervations themſelves; for the firſt obſervation, ſet down as taken with this inſtrument, being upon the 29th of October, 1676, it was not till the 11th of September, 1677, that the column which contained the *check* angle by diagonal lines was filled up; and there was alſo a ſpace of time, antecedent to that laſt mentioned, wherein no obſervations are recorded as taken with this inſtrument, in which time the diagonal diviſions might be put on; and this will be put beyond a doubt, as he ſays expreſſly, that finding, in the year 1677, that the threads of the ſcrew had worn the border of the limb, he divided the limb into degrees himſelf, and drew a ſet of diagonal diviſions ‡; and then comparing the two ſets of diviſions together, he ſometimes found them to differ a whole minute; wherefore, for correction thereof, he conſtructed a new table for converſion of the revolutions and parts of the ſcrew into degrees, minutes, and ſeconds; and which he applied in the obſervations taken in 1678.—However, notwithſtanding this correction, in looking over the obſervations noted down as deduced each way, I frequently find a difference of half a minute; not unfrequently 40″; but in an obſervation of the moon, of the 9th June, 1687, I find a difference of 55″[*], which upon a radius of 6 feet 9 inches amounts to more than $\frac{1}{30}$th part of an inch.

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* "—Qualem nemo, cœlo adhibens;—" Preface to Hiſtoria Cœleſt. printed in one vol. 1712.

† Prolegomena Hiſtor. Cœleſt. vol. III. p. 104.

‡ Ibid. p. 106. "Gradus in limbo deſtribui ac diagonales duxi."

In the year 1689, Mr. FLAMSTEED compleated his *mural arc* at GREENWICH; and, in the *Prolegomena* before-mentioned, he makes an ample acknowledgement of the particular aſſiſtance, care, and induſtry of Mr. ABRAHAM SHARP; whom, in the month of Auguſt, 1688, he brought into the obſervatory, as his *amanuenſis*; and being, as Mr. FLAMSTEED tells us, not only a very ſkilful mathematician, but exceedingly expert in mechanical operations [†], he was principally employed in the conſtruction of the mural arc; which in the compaſs of fourteen months he finiſhed, ſo greatly to the ſatisfaction of Mr. FLAMSTEED, that he ſpeaks of him in the higheſt terms of praiſe [‡].

This celebrated Inſtrument, of which he alſo gives the figure at the end of the *Prolegomena*, was of the radius of 6 feet 7½ inches; and, in like manner as the ſextant, was furniſhed both with ſcrew and diagonal diviſions, all performed by the accurate hand of Mr. SHARP. But yet, whoever compares the different parts of the table for converſion of the revolutions and parts of the ſcrew belonging to the mural arc into degrees minutes and ſeconds *, with each other, at the ſame diſtance from the zenith on different ſides; and with their halves, quarters, &c. will find as notable a diſagreement of the ſcrew-work from the hand-diviſions, as had appeared before in the work of Mr. TOMPION: and hence we may conclude, that the method of Dr. HOOK, being executed by two ſuch maſterly hands as TOMPION and SHARP, and found defective, is in reality not to be depended upon in nice matters.

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[*] Vol. I. of Hiſt. Cœleſt. p. 343.

[†] "Qui mechanices perquam expertus, pariter ac matheſeos peritus." Prolegomena, vol. III. p. 108.

[‡] "SHARPEIUS ſervus meus fideliſſimus, ac omnibus quidem dotibus & facul-
" tatibus erat imbutus, quæ ipſum tam ſubtili & difficili operi obeundo idoneum
" redderent." Prolegom. ibid.

And on finiſhing the inſtrument, he ſays, "Gradus deſcribuntur ſive nume-
" rantur et exſculpantur, artificioſa manuali opera dicti domini SHARP, qui limbum
" partitus eſt, diagonales duxit, totumque organum abſolvit et perfecit: adeo
" ut præſtantiſſimi quivis artifices poſtquam illud conſpexerunt et conſiderarunt,
" ſe exactius id peragere non potuiſſe, agnoverint." Prolegom. p. 111.

From the account of Mr. FLAMSTEED it appears alſo, that Mr. SHARP obtained the zenith point of the inſtrument, or *line of collimation*, by obſervation of the zenith ſtars, with the face of the inſtrument on the eaſt and on the weſt ſide of the wall †: and that having made the index ſtronger (to prevent flexure) than that of the ſextant, and thereby heavier, he contrived, by means of pullies and balancing weights, to relieve the hand that was to move it from a great part of its gravity †.

I have been the more particular relating to Mr. SHARP, in the buſineſs of conſtructing this mural arc; not only becauſe we may ſuppoſe it the firſt good and valid inſtrument of the kind, but becauſe I look upon Mr. SHARP to have been the firſt perſon that cut accurate and delicate diviſions upon aſtronomical inſtruments; of which, independent of Mr. FLAMSTEED's teſtimony, there ſtill remain conſiderable proofs: for, after leaving Mr. FLAMSTEED, and quitting the department above-mentioned ‡, he retired into *Yorkſhire*, to the village of *Little Horton*, near *Bradford*, where he ended his days about the year 1743; and where I have ſeen not only a large and very fine collection of mechanical tools (the principal ones being made with his own hands), but alſo a great variety of ſcales and inſtruments made therewith, both in wood and braſs, the diviſions whereof were ſo exquiſite, as would not diſcredit the firſt artiſts of the preſent times: and I believe there is now remaining a quadrant, of four or five feet radius, framed of wood, but the limb covered with a braſs plate; the ſubdiviſions being done by diagonals, the lines of which are as finely cut as thoſe upon the quadrants at Greenwich. The delicacy of Mr. SHARP's hand will indeed permanently appear from the copper-plates in a quarto book, publiſhed in the year 1718, intituled, *Geometry improved by A. Sharp, Philomath.* whereof not only the geometrical lines upon the plates, but the whole of the engraving of letters and figures, were done by himſelf, as I was told by a perſon in the mathematical line, who very frequently attended Mr. SHARP in the latter part of his life. I therefore look upon Mr. SHARP as the firſt perſon that brought the affair of hand diviſion to any degree of perfection.

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* Hiſt. Cœleſt. vol. II. Appendix. † Prolegom. p. 109.

‡ Mr. SHARP continued in ſtrict correſpondence with Mr. FLAMSTEED ſo long as he lived, as appeared by letters of Mr. FLAMSTEED's found after Mr. SHARP's death; many of which I have ſeen.

Some time about the eſtabliſhment of the mural arc at Greenwich, the celebrated Daniſh Aſtronomer OLAUS ROEMER began his domeſtic Obſervatory, which he finiſhed in the year 1715, as we are informed by his hiſtorian PETER HORREBOW, in the third volume of his works, in the tract, intituled, *Baſis Aſtronomiæ*, publiſhed in the year 1741. In this tract is the deſcription of an inſtrument, Tab. III. which not only anſwered the purpoſe of the meridian arc; but, its teleſcope being mounted upon a long axis, became alſo in reality what we now call a *Tranſit Inſtrument*; and which furniſhed, ſo far as I have been able to learn, the firſt idea thereof. One end of the axis of this inſtrument being the center of the meridian arc, and carrying its index, M. ROEMER thereby avoided the errors ariſing from the plane of the mural arc not being accurately a vertical plane; and which Mr. FLAMSTEED endeavoured to check, by obſerving the paſſage of known ſtars nearly in the ſame parallel of declination; that is, paſſing nearly over the ſame part of the plane of the arc; by which he was enabled to correct or check the errors of the arc in right aſcenſion. But it is the peculiar method in which ROEMER *divided* his inſtruments, that occaſions him here to be introduced.

Though it is a very ſimple problem by which geometricians teach how to divide a given right line into any number of parts required; yet it is ſtill a much more ſimple thing to ſet off upon a given right line, from a point given, any number of equal parts required, where the total length is not exactly limited; for this amounts to nothing more than aſſuming a convenient opening of the compaſſes, and beginning at the point given, to ſet off the opening of the compaſſes as many times in ſucceſſion, as there are equal parts required; which proceſs is as applicable to the arch of a circle as it is to a right line. Of this ſimple principle ROEMER endeavoured to avail himſelf.

For this purpoſe M. ROEMER took two ſtiff, but very fine-pointed, pieces of ſteel, and fixed them together, ſo as to avoid, as much as poſſible, every degree of ſpring that would neceſſarily attend long-legged compaſſes, or even thoſe of the ſhorteſt and ſtiffeſt kind when the points are brought near together. The diſtance of the points that he choſe was about the $\frac{1}{10}$ or $\frac{1}{11}$ of an inch. This, upon a radius of 2½ or three feet, would be about 10 minutes. With this opening, beginning at the point given, he ſet off equal ſpaces in ſucceſſion to the end of his arch, which was about 75°. Thoſe were diſtinguiſhed upon the limb of the inſtrument by very fine points, which were referred to by a groſſer diviſion, the whole being properly numbered. The ſubdiviſion of thoſe arches of 10 minutes each was performed by a double microſcope, carried upon the arm or radius of the inſtrument, the common focus being furniſhed with parallel threads of ſingle ſilk, whereof eleven being diſpoſed at ten equal intervals, comprehending together one ten-minute diviſion, the diſtance of the neareſt threads became a very viſible ſpace, anſwerable to one minute each, and therefore capable of a much further ſubdiviſion by eſtimation.

The diviſions of this inſtrument were therefore, properly ſpeaking, not degrees and minutes; but yet, if exactly equal, would ſerve the purpoſe as well, when their true value was found, which was done by compariſon with larger inſtruments.

Now, if it be conſidered, that in going ſtep by ſtep of ten minutes each, through a ſpace of 75 degrees, there will be a ſucceſſion of 450 diviſions, dependant upon each other; if it be alſo conſidered, that the leaſt degree of extuberance in the ſurface of the metal, where each new point is ſet down, or the leaſt hard particle (wherewith all the baſe metals ſeem to abound) will cauſe a deviation in the firſt impreſſion of a taper point, and thereby produce an inequality in the diviſion; it is evident, that though this inequality may be very ſmall, and even imperceptible between neighbouring diviſions, yet among diſtant ones, it may and will ariſe to ſomething conſiderable; which, in the menſuration of angles, will have the ſame ill tendency as in near ones. Now, as M. Roemer has given us no means of checking the diſtant diviſions, in reſpect of each other, it is very probable that no one has followed his ſteps, in caſes where great accuracy was required, in a conſiderable number of diviſions. For in reality this method is likely to fall far ſhort of Dr. HOOK's; as Dr. HOOK's diviſions being cut in a ſimilar ſucceſſive manner, by the rotation of the ſharp *edge* of the threads of a ſcrew againſt the exterior edge of the limb of the inſtrument, a very ſlight degree of preſſure will bring a fine ſcrew of thirty threads in an inch (which he preſcribes) to touch againſt an arch whoſe radius is four or five feet in more than one, two, three, or four threads at once; ſo that the threads ſupporting one another, a ſmall extuberance, or even a ſmall hard particle in the metal, will be cut through or removed by the grinding or rather ſawing motion of the ſcrew; and which, in regard to its contact, being in reality an edge, will be much more effectual (that is, more firm) in its retention than a mere ſimple point: and a repetition of the operation, from the ſupport of the threads to each other, will tend to mend the firſt traces; whereas, in ROEMER's way, a repetition will make them worſe; for, whatever drove forward or backward the point on firſt entering, will, from the ſloping of the point, be confirmed and increaſed in driving it deeper.

When Dr. HALLEY was choſen Aſtronomer Royal (Mr. FLAMSTEED's inſtruments being taken away by his executors), Mr. GRAHAM undertook to make a new mural quadrant, about the year 1725; who, uniting all that appeared valuable in the different methods of his predeceſſors, executed it with a degree of contrivance, accuracy, and preciſion, before unknown: and the diviſion thereof he performed with his own hand. The model of this quadrant, for ſtrength, eaſy management, and convenience, has been ever ſince purſued as the moſt perfect.

What I apprehend to be peculiar in it, was the application of the arch of 96°; not only as a check upon the arc of degrees and minutes, but as ſuperior thereto, being derived from the more ſimple principle of *continual biſection.*

To make room for this, he has entirely rejected the ſubdiviſion by diagonals, and has adopted the method of the *Vernier*; but the ſubdiviſion of the vernier diviſions he, as I apprehend for the firſt time, meaſured by the turns of the detached adjuſting ſcrew, making it in fact a micrometer, by which the diſtance of the *ſet* of the inſtrument was to be meaſured from the perfect coincidence of one of the actual diviſions of the limb with the next ſtroke of the vernier; by which means the obſervation could not only be read off with all the preciſion that the diviſion of the inſtrument was capable of, but the two ſets of diviſions could be checked and compared with each other. Another thing that I apprehend to be peculiar in this inſtrument, was the more certain method of transferring and cutting the diviſions, from the original divided points, by means of the *beam-compaſs*, than could poſſibly be done from a *fiducial edge*, as had doubtleſs been conſtantly the practice in cutting diagonals; for, placing the ſteady point of the beam-compaſs in the tangent line to that part of the arc where each diviſion was to be cut, the opening of the compaſs being nearly the length of the tangent, the other point would cut the diviſion in the direction of the radius nearly; and though in reality an arch of a circle, yet the ſmall part of it in uſe would be ſo nearly a right line, as perfectly to anſwer the ſame end; all which advantages put together, it is probable, induced Mr. **Graham** to reject the diagonals.

Soon after the completion of this quadrant, Mr. **Graham** undertook to execute a *zenith ſector* for the Rev. Dr. **Bradley**, which was fixed up at *Wanſtead*, in *Eſſex*, in the year 1727. The very ſimple conſtruction that he adopted for this inſtrument (the plumb line itſelf being the index) did not admit of the uſe of a vernier: he therefore contented himſelf with dividing the arch of the limb of this inſtrument by primary points, as cloſe as he thought neceſſary, that is, by diviſions of five minutes each, and meaſuring the diſtance from the *ſet* of the inſtrument to the next point of diviſion by a *micrometer* ſcrew, in the conſtruction of which ſcrew he uſed uncommon care and delicacy. I have mentioned this inſtrument to introduce this obſervation; that I think it highly probable, had Mr. GRAHAM conſtructed the great quadrant *after* the zenith ſector had been fully tried, he would have rejected not only the diagonals but the verniers alſo, as containing a ſource of error within themſelves which may be avoided by a well-made ſcrew *.

It ſeems alſo, that Mr. GRAHAM, at the time he conſtructed both theſe inſtruments, was not aware how much error could ariſe from the unequal expanſions of different metals by heat or cold: for in both the radius, or frame of the inſtrument, was iron, while the limbs were of braſs. They, however, remain in the Royal Obſervatory, perfect models, in all other reſpects, of every thing that is likely to be attained in their reſpective deſtinations, and monuments of the ſuperlative abilities of that great mechanician Mr. GRAHAM †.

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* This has been found conſentaneous to the experience of my friend Mr. AUBERT, who, on my ſuggeſtion, has long ſince laid aſide the uſe of his vernier, meaſuring always by the micrometer ſcrews the diſtance between the ſet of the inſtrument, and the coincidence of the firſt ſtroke of the vernier with the next primary diviſion of the limb.

† I have been informed, that Dr. MASKELYNE has cauſed this objection to the ſector to be rectified, ſince its removal to the Royal Obſervatory, by ſubſtituting an iron limb inſtead of that of braſs, the points being made upon ſtuds of gold.

Mr. GRAHAM lived till the year 1751; and during his time there were few inſtruments of conſequence conſtructed without his advice and opinion. They were for many years done by Mr. SISSON, to whom doubtleſs Mr. GRAHAM would fully communicate his method of diviſion; and from this ſchool aroſe that very eminent and accurate artiſt Mr. BIRD, whoſe delicate hand, joined with great care and aſſiduity, enabled him ſtill further to promote this branch of diviſion; and which being carried by him to a great pitch of perfection, the Commiſſioners of Longitude did themſelves the credit, by an handſome reward, to induce him to publiſh to the world his particular method of dividing aſtronomical inſtruments; which being drawn up by himſelf, in the year 1767, this matter is fully ſet forth to the public: I ſhall therefore only take this opportunity of obſerving, that there ſeems to be one article in which Mr. BIRD's method may be ſtill improved.

I muſt here obſerve, that I apprehend no quadrant, that has ever undergone a ſevere examination, has been found to form a perfect arch of 90°; nor is it at all neceſſary it ſhould: the perfect equality of the diviſions throughout the whole is the firſt and primary conſideration; as the proportion of error, when aſcertained by proper obſervations, can be as eaſily and readily applied, when the whole error of the rectangle is fifteen ſeconds, as when it is but five.

In this view, from the radius taken, I would compute the chord of ſixteen degrees only. If I had an excellent plain ſcale, I would uſe it; becauſe I ſhould expect the deviation from the right angle to be leſs than if taken from a ſcale of more moderate accuracy; but if not, the equality of the diviſions would not be affected, though taken from any common diagonal ſcale. This chord, ſo prepared, I would lay off five times in ſucceſſion, from the primary point of o given, which would compleat eighty degrees; I would then biſect each of thoſe arches of 16°, as preſcribed by Mr. BIRD, and laying off one of them beyond the 80th, would give the 88th degree: proceeding then by biſection, till I came to an arch of two degrees, laying that off from the 88th degree, would give the point of ninety degrees. Proceeding ſtill by biſection, till I had reduced the degrees into quarters = fifteen minutes each, I would there ſtop; as from experience I know, that when diviſions are over cloſe, the accuracy of them, even by biſection, cannot be ſo well attained as where they are moderately large. If a ſpace of $\frac{4}{10}$ of an inch, which is a quarter of a degree, upon an eight-feet radius, is thought too large an interval to draw the index over by the micrometer ſcrew, this may be ſhortened by placing another line at the diſtance of one-third of a diviſion on each ſide of the index line, in which caſe the ſcrew will never have to move the index plate more than one-third of a diviſion, or five minutes; and the perfect equality of thoſe ſide lines from the index line may be obtained, and adjuſted to five minutes preciſely, by putting each of the ſide lines upon a little plate, capable of adjuſtment to its true diſtance from the middle one, by an adjuſting ſcrew.

The above hint is not confined to the chord of ſixteen degrees, which prohibits the ſubdiviſions going lower than fifteen minutes: for if it be required to have diviſions equivalent to five minutes upon the limb itſelf; then I would compute the chord of 21° 20′ *only*; and laying it off four times from the primary point, the laſt would mark out the diviſion 85° 20′, pointed out by Mr. BIRD; *ſupplying the remainder* to a quadrant, from the biſected diviſions as they ariſe, and not by the application of other *computed* chords.

In my Introduction to M. Roemer's Method of Diviſion, I have ſhewn, that diviſions laid off in ſucceſſion, by the ſame opening of the compaſſes, either in a right line, or in the arch of a circle, being in its idea geometrically true, and in itſelf the moſt ſimple of all proceſſes, it has the faireſt chance of being mechanically and practically exact, when cleared of the diſturbing cauſes. The objection therefore to his method is, the great number of repetitions, which depending upon each other in ſucceſſion (requiring no leſs than 540 to a quadrant, when ſubdivided to ten minutes each), the ſmalleſt error in each, repeated 540 times, without any thing to check it by the way, may ariſe to a very ſenſible and large amount: but in the method I have hinted, this objection will not lie; for, in the firſt caſe, the aſſumed opening is laid off but five times; and in the latter caſe but four times; nor does this *repetition* ariſe out of the nature of the thing; for, if you like it better, you may, in the former caſe, at once compute the chord of 64°; and in the latter that of 85° 20′, and then proceed wholly by biſection; ſupplying what is wanted to make up the quadrant, from the biſected diviſions, as they ariſe. Mr. Bird preſcribes this method himſelf, for the diviſion of Hadley's ſextants and octants.

He, I ſuppoſe, was the firſt, who conceived the idea of laying off chords of arches, whoſe ſubdiviſions ſhould be come at by continual biſection; but why he mixed therewith diviſions that were derived from a different origin (as preſcribed in his method of dividing) I do not eaſily conceive. He ſays, that after he had proceeded by the biſections, from the arc of 85° 20′, the ſeveral points of 30°, 60°, 75°, and 90°, (all of which were laid down from the principle of the chord of 60° being equal to radius), *fell in without ſenſible inequality*; and ſo indeed they might; but yet it does not follow that they were equally true in their places as if they had been (like the reſt) laid down from the biſection from 85° 20′, and therefore being the firſt made, whatever error was in them, would be communicated to all connected with them, or taking their departure from them. Every heterogeneous mixture ſhould be avoided where equal diviſions are required. It is not the ſame thing (as every good artiſt will ſee) whether you *twice* take a meaſure from a ſcale as *nearly* the ſame as you can, and lay them off ſeparately; or lay off *two* openings of the compaſſes, in ſucceſſion, *unaltered*; for though the ſame opening, carefully taken off from the ſame ſcale a ſecond time, will doubtleſs fall into the points made by the firſt, without ſenſible error; yet as the ſloping ſides of the conical cavities made by the firſt point will conduct the points themſelves to the center, there may be an error which, though inſenſible to the ſight, would have been avoided by the *more ſimple proceſs* of laying off the opening twice, without ever altering the compaſſes.

The 96 arc was I have no doubt, invented by Mr. GRAHAM, from having perceived, in common with all preceding artiſts, how very much more eaſy a given line was to biſect, than to triſect, or quinqueſect; and therefore the 96 arc which proceeded by biſections only (or by laying off the ſame identical openings, which, as already ſhewn, is ſtill more ſimple and unexceptionable) was wholly intended by him by way of checking the diviſion of the arc of 90, which required triſections and quinqueſections. But experience ſoon ſhewed the ſuperior advantage of it ſo ſtrongly, that the uſe of the 90 arc is now wholly ſet aſide, where accuracy is required; whereas the ingenuity of Mr. BIRD having ſhewn a way to produce the 90 arc by biſection, when this is really purſued quite through the piece, by rejecting all diviſions derived from any other origin, the 90 arc will have nothing in it to prevent its being equally unexceptionable with the 96 arc; and conſequently if, inſtead of the 96 arc, another arc of 90 was laid down (which being upon a different radius, its diviſions will ſtand totally unconnected with the former), then theſe two arcs would in *reality* be a check upon each other; for being of equal validity, the mean might be taken: and if (in lieu of vernier diviſions) ſtrokes at the diſtance of any odd number, as 7, 9, 11, or 13, are marked upon, and carried along with, the index plate; theſe will produce a check upon neighbouring diviſions; and the angle may then be deduced from the medium of no leſs than four readings.

The laſt works that have been made known to the public in the line of graduation (ſo far as has come to my knowledge) are thoſe of the very ingenious Mr. RAMSDEN, which were publiſhed, by order of the Board of Longitude, in the year 1777.

From his own information, I learn, that in the year 1760 he turned his thoughts towards making an engine for dividing mathematical inſtruments; and this he did in conſequence of a reward offered by the Board of Longitude to Mr. BIRD, for publiſhing his method of graduating quadrants; for as, ſeveral years previous to that period, he had taken great pains to accompliſh himſelf in the art of hand-dividing, in which line Mr. BIRD had acquired his eminence, he conceived by this publication of Mr. BIRD's he ſhould be reduced to the ſame ſtandard of performance with the reſt of the trade. He, therefore, partly to ſave time, and that kind of wearineſs to an ingenious mind that ever muſt attend the endleſs repetition of the ſame thing from morning to night; partly ſtill to preſerve the pre-eminence he had then gained; and partly to procure diſpatch in the great increaſe of demand for HADLEY's ſextants and octants, in conſequence of the ſucceſsful application of the moon's motion to the purpoſe of aſcertaining the longitude at ſea (which inſtruments for this purpoſe required a degree of accuracy and certainty in the diviſion, by no means neceſſary thereto when applied to the ſimple purpoſe of obſerving latitudes); I ſay, for theſe conſiderations, Mr. RAMSDEN determined to ſet about ſomething in the inſtrumental way, that ſhould be ſufficient effectually to anſwer theſe purpoſes.

Accordingly, conſidering the nature of the endleſs ſcrew, he ſet himſelf to work upon an engine whoſe divided wheel or plate was of thirty inches diameter; and though the performance of this firſt eſſay was inferior to his expectations and wiſhes, yet with it he was able to divide theodolites with a degree of preciſion far ſuperior to any thing of the kind that had been exhibited to the public.

This engine I myſelf ſaw in the ſpring of the year 1768; and it appeared to me not only a very laudable attempt towards inſtrumental diviſions, but a very good model for the conſtruction of an engine of the moſt accurate kind for that purpoſe. And he furthermore, at the ſame time, ſhewed me the model or pattern for caſting a wheel of a much larger ſize, which he propoſed to make upon the ſame plan, and with conſiderable improvements. This being effected ſome time in or about the year 1774, its accuracy was proved by making a ſextant, afterwards ſubjected to the examination of Mr. BIRD; who in conſequence approved the method, not only as fully ſufficient for the diviſion of HADLEY's ſextants and octants for any purpoſe whatſoever, but in fact for dividing any inſtrument whoſe radius did not exceed that of the dividing wheel, which was forty-five inches in diameter: whereupon the Board of Longitude, ever ready to encourage all endeavours that tend to the certainty and perfection of any thing ſubſervient to the purpoſe of finding the longitude at ſea, very properly and uſefully reſolved to confer an handſome reward on Mr. RAMSDEN, for delivering a full explanation of his method of making the ſaid engine; which, in conſequence, was publiſhed by order of the Board of Longitude in the year already mentioned, viz. 1777: the deſigns whereof are ſo full and explicit, that whoever could not underſtand that deſcription, ſo as to enable him to make it, would be unfit to undertake it on other accounts.

From what I have ſaid upon the works of the different artiſts that I have mentioned, it would ſeem that the art of graduation was brought to that degree of perfection, that nothing material can now be added thereto: and I ſhould have been apt to have thought ſo myſelf, if I had not happened, in the courſe of my life, to have had a communication made to me (under the ſeal of ſecrecy) which ſeems to promiſe yet further light and aſſiſtance in perfecting that important art; and every impediment to the diſcovery thereof being now removed, I ſhall in the remainder of this eſſay give the cleareſt deſcription thereof that I am able, with ſuch elucidations and improvements as ſeem to be naturally pointed out by the method itſelf.

In the autumn of the year 1741, I was firſt introduced to the acquaintance of that then eminent artiſt, Mr. HENRY HINDLEY, of *York*, clockmaker;—he immediately entered with me into the greateſt freedom of communication, which founded a friendſhip that laſted to his death, which did not happen till the year 1771, at the age of 70. On the firſt interview, he ſhewed me not only his general ſet of tools, but his *engine*, at that time furniſhed with a dividing plate, with a great variety of numbers for cutting the teeth of clock wheels, &c. and alſo, for more nice and curious purpoſes, furniſhed with a wheel of about thirteen inches diameter, very ſtout and ſtrong, and cut into 360 teeth; to which was applied an endleſs ſcrew, adapted thereto. The

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threads of this ſcrew were not formed upon a cylindric ſurface, but upon a ſolid whoſe ſides were terminated by arches of circles. The whole length contained fifteen threads; and as every thread (on the ſide next the wheel) pointed towards the center thereof, the whole fifteen were in contact together; and had been ſo ground with the wheel, that, to my great aſtoniſhment, I found the ſcrew would turn round with the utmoſt freedom, interlocked with the teeth of the wheel, and would draw the wheel round without any ſhake or ſticking, or the leaſt ſenſation of inequality.

How long this engine might have been made before this firſt interview, I cannot now exactly aſcertain: I believe not more than about a couple of years; but this I well remember, that he then ſhewed me an inſtrument intended for aſtronomical purpoſes, which muſt have been produced from the engine, and which of itſelf muſt have taken ſome time in making*.

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* This inſtrument was of the equatorial kind; the wheel parallel to the equator, the quadrant of latitude, and ſemi-circle of declination, being all furniſhed with ſcrews containing fifteen threads each, framed and moved in the ſame manner as that of the engine; the whole of which inſtrument was already framed, and the teleſcope tube in its place, which was intended to be of the inverting refracting kind, and to be furniſhed with a micrometer. This, however, was not compleated till ſome years after; but, in the year 1748, I received it in London for ſale. It ſtaid with me two years, in which time I ſhewed it to all my mechanical and philoſophical friends, amongſt whom was Mr. SHORT, who afterwards publiſhed in the Philoſophical Tranſactions, vol. XLVI. Nº 493. p. 241. an Account of a portable Obſervatory, but without claiming any particular merit from the contrivance. However, the model of it differs from HINDLEY's equatorial only in the following articles. He added an azimuth circle and compaſs at the bottom. He omitted the endleſs ſcrews, placing verniers in their ſtead; and at the top, a reflecting teleſcope inſtead of a refractor. This inſtrument of HINDLEY's being afterwards returned to him unſold, I pointed out the principal deficiencies that I found therein; viz. that, in putting the inſtrument into different poſi-

tions,

I in reality thought myſelf much indebted to Mr. HINDLEY for this communication; but though he ſhewed me his engine, and told me, that the ſcrew was cut by the rotation of the point of a tool, carried round upon a ſtrong arm, at the diſtance of the radius of the wheel from the center of motion, which arm was carried forward by the wheel itſelf, and the wheel was put forward by an endleſs ſcrew, formed upon a cylinder to a proper ſize of thread, cut by his chock lathe; though he ſhewed me alſo this chock lathe, and the method employed to make the threads of the ſcrew *equiangular* with the axis, that is, to free the ſcrew from what workmen term *drunkenneſs*; and alſo ſhewed me how, by the ſingle ſcrew of his lathe, he could cut, by means of wheel-work, ſcrews of every neceſſary degree of fineneſs [*] (and, by taking out a wheel, could cut a left-handed ſcrew of the very ſame degree of fineneſs); by which means he was enabled not only to adapt his plain ſcrew to the ſize of the teeth of his wheel, but alſo to prevent any drunkenneſs that otherwiſe the curved ſcrew would be ſubject to in conſequence of being produced from the plain

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tions, the ſpringing of the materials was ſuch as in ſome poſitions to amount to conſiderable errors. This remained with him in the ſame ſtate till the year of the firſt *Tranſit of Venus*, viz. 1761; when it was ſold to —— CONSTABLE, Eſq. of *Burton Conſtable*, in *Holderneſs*. Mr. HINDLEY, to remedy the evil above-mentioned, applied balances to the different movements. He ſoon afterwards completed one, *de novo*, upon this improved plan, for his Grace the late Duke of NORFOLK. A method of balancing in much the ſame way, without the knowledge that it had been done before, has been fully explained, and laid before the Society, by our ingenious and worthy brother Mr. NAIRNE. Phil. Tranſ. vol. LXI. p. 108.

[*] A machine for cutting the endleſs ſcrew of Mr. RAMSDEN's engine, upon principles exactly ſimilar, is fully and accurately ſet forth in his Deſcription of his dividing Engine above-mentioned.

one; furthermore, that the ſcrew and wheel, being ground together as an optic glaſs to its tool, produced that degree of ſmoothneſs in its motion that I obſerved; and, laſtly, that the wheel was cut from the dividing plate: yet, how the dividing plate was produced, he for particular reaſons reſerved to himſelf.

Nor can he be blamed for the reſervation of this one ſecret; as he had, even at the time of my early acquaintance with him, a kind of foreſight that from the ſuperior merit of HADLEY's quadrant, a demand for that, and other inſtruments for the purpoſe of navigation, was likely to increaſe; and that he might live to ſee a public reward offered for a method of dividing them with greater accuracy and diſpatch than had at that time appeared. Indeed, he had himſelf an idea, from the ſatisfactory ſucceſs that had attended his operations in dividing, that a ſcrew and wheel, produced from his engines of one foot diameter, would have as much truth as the eight-feet quadrant at Greenwich: and though he doubtleſs greatly over-rated the accuracy of theſe miniature performances, yet it does not follow, as his methods were not confined to ſo narrow a compaſs, but that, his ſcale of operation being proportionably enlarged, a degree of accuracy in the graduation of aſtronomical inſtruments may be attained in proportion.

I muſt here beg leave to obſerve, that there appears to me to be a natural limitation to the accuracy of inſtruments, conſiſting of conſiderable portions of a circle, ſuch as quadrants, &c.*. I do not find that the fineſt ſtroke upon the limb of a quadrant, made by BIRD's own hand, if removed from its

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* The zenith ſector conſiſts but of *few* degrees, with little variation of its poſition in uſing it.

coincidence

coincidence with its index, can be replaced with any degree of certainty nearer than the 4000dth part of an inch, though aided by a magnifying glaſs *.

A 4000dth part of an inch being then determined to be the *minimum viſibile* by the ſtrokes of an inſtrument, this will be leſs than one ſecond of a degree upon a radius of four feet; and therefore, if the whole ſet of diviſions upon the limb could be preſerved true to this aliquot part of an inch, the eight-feet quadrants of Greenwich might be expected to be true to half a ſecond. How far they are from this, I do not exactly know; but I have reaſon to think they vary from it ſome ſeconds: nay, I believe it is generally allowed, that our largeſt quadrants, even when executed by the accurate hand of Mr. BIRD, do not exceed thoſe of a leſs ſize, by the ſame hand, in proportion to their increaſe of radius: nor can it well be expected that they ſhould; ſince, as the weight neceſſarily increaſes in a triplicate ratio of the radius, the great weight of the Greenwich quadrants in moving and fixing them (as they could not be divided in their place) may eaſily derange the framing; or even the *internal elaſticity* of the materials may give way, by a change of poſition, to ſo minute a quantity as a 4000dth part of an inch. It therefore appears to me, that ſince the diviſions of a quadrant of four-feet radius are more than ſufficient, and even thoſe of three feet admit of all the diſtinctneſs that in other reſpects is wanted, a three-feet quadrant, in point of ſize, is capable of all attainable exactneſs; and would be as much to be depended on as any of thoſe now in being of eight feet. By adopting quadrants of this ſmaller ſize, we ſhall of courſe get rid of $\frac{18}{19}$ths of the preſent weight; and conſequently of much cumber, unhandineſs, and derangement, that muſt ariſe from that weight, as well as the fear of totally diſcompoſing them, if ever moved out of their place.

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* It will be to little purpoſe to attempt it with a greater power. Double microſcopes can doubtleſs be formed to magnify objects, far leſs than a 4000dth part of an inch, to diſtinct ſurfaces; but then the advantage of ſuch degrees of magnifying power is chiefly upon the organized bodies of nature. Let a dot, or the fineſt point that can be made by human art, be ſo viewed, and it will appear not round, but a very ragged irregular figure.

It now comes to be time to open a principle upon which there is a proſpect of effecting ſuch an improvement. I have ſhewn that a 4000th part of an inch is the *ultimatum* that we are to expect from *ſight*, though aided by glaſſes, when obſerving the diviſions of an inſtrument. But in the XLVIIIth volume of the Philoſophical Tranſactions for the year 1754, I have ſhewn the mechaniſm of a new *pyrometer*, and experiments made therewith; whereby it appears, that, upon the principle of *contact*, a 24,000th part of an inch is a very definite quantity. I remembered very well that I did not then go to the extent of what I might have aſſerted, being willing to keep within the bounds of *credibility*: but on occaſion of the preſent ſubject, I have re-examined this inſtrument, and find myſelf very well authoriſed to ſay, that a 60,000th part of an inch, with ſuch an inſtrument, is a more definite and certain quantity than a 4000th part of an inch is to the *ſight*, conditioned as above ſpecified. The certainty of contact is, therefore, fifteen times greater than that of viſion, when applied to the diviſions of an inſtrument; and if this principle of certainty in contact did not take place even much beyond the limit I have now aſſigned, we never ſhould have ſeen thoſe exquiſite mirrors for reflecting teleſcopes, that have already been produced.

Theſe reflections apply immediately to my preſent ſubject, as HINDLEY's method of diviſion proceeds *wholly by contact*, and that of the firmeſt kind; there being ſcarcely need of magnifying glaſſes in any part of the operation.

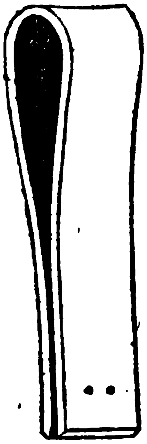
In the year 1748 I came to ſettle in London; and the firſt employment I met with was that of making philoſophical inſtruments and apparatus. In this ſituation, my friend HINDLEY, from a principle the reverſe of jealouſy, fully communicated to me, by letter, his method of diviſion; and though I was enjoined ſecrecy reſpecting others (for the reaſons already mentioned), yet the communication was expreſsly made with an intention that I might apply it to my own purpoſes.

The following are extracts from two letters, which contain the whole of what related to this ſubject; and ſince I have many things to obſerve thereon, ſo that the paraphraſe would be much greater than the text, I think it beſt not to interrupt the deſcription with any commentary, as perhaps his own mode of expreſſion will more briefly and happily convey the general idea of the work than any I can uſe inſtead of it.

MY DEAR FRIEND, York, 14 Nov. 1748.

AS to what you was mentioning about my brother's knowing how I divided my engine plate, I will deſcribe it as well as I can myſelf; but you will want a good many things to go through with it.

The manner is this: firſt chuſe the largeſt number you want, and then chuſe a long plate of thin braſs; mine was about one inch in breadth, and eight feet in length, which I bent like an hoop for an hogſhead, and ſoldered the ends together; and turned it of equal thickneſs, upon a block of ſmooth-grained wood, upon my great lathe in the air (that is, upon the end of the mandrel): one ſide of the hoop muſt be rather wider than the other, that it may fit the better to the block, which will be a ſhort piece of a cone of a large diameter: when the hoop was turned, I took it off, cut, and opened it ſtraight again.



The next ſtep was to have a piece of ſteel bended into the form as *per margin* *; which had two ſmall holes bored in it, of equal bigneſs, one to receive a ſmall pin, and the other a drill of equal ſize. I ground the holes after they were hardened, to make them round and ſmooth. The chaps formed by this ſteel plate were as near together as juſt to let the long plate through. Being open at one end, the chaps ſo formed would ſpring a little, and would preſs the long plate cloſe, by ſetting in the *viſe*. Then I put the long plate to a right angle to the length of the ſteel chaps, and bored one hole through the long plate, into which I put the ſmall pin; then bored through the other hole; and by moving the ſteel chaps a hole forward, and putting in the pin in the laſt hole, I proceeded till I had divided the whole length of the plate.

The next thing was to make this into a circle again. After the plate was cut off at the end of the intended number, I then proceeded to join the ends, which I did thus: I bored two narrow ſhort braſs plates † as I did the long one, and put one on the inſide, and the other on the outſide of the hoop, whoſe ends were brought together; and put two or three turned ſcrew pins, with flat heads and nuts to them, into each end, which held them together till I rivetted two little plates, one on each ſide of the narrow plate, on the outſide of the hoop. Then I took out the ſcrews, and turned my block down, till the hoop would fit cloſe on; and by that means my right line was made into an equal divided circle of what number I pleaſed.

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* The figure is conſiderably leſs than the real tool ſhould be.

† Theſe I ſhall hereafter diſtinguiſh by the name of *ſaddle-plates*.

The engine plate was fixed on the face of the block, with a ſteel hole fixed before it, to bore through; and I had a point that would fall into the holes of the divided hoop; ſo by cutting ſhorter, and turning the block leſs, I got all the numbers on my plate.

I need not tell you, that you get as many prime numbers as you pleaſe; nor that the diſtance of the holes in the ſteel chaps muſt be proportioned to the length of the hoop.

You may aſk my brother what he knows about my method of dividing; but need not tell him what I have ſaid about it; for I think neither *he* nor *John Smith* know ſo much as I have *told you*, though I believe they got ſome knowledge of it in general terms *.——I deſire you to keep the method of dividing to *yourſelf*, and conclude with my beſt wiſhes,

and am, dear Sir, yours, &c.

HEN. HINDLEY.

Though the above letter was in itſelf very clear and explicit, as to the general traces of the method, yet ſome doubts occurring to me, a further explanation became neceſſary. A copy of my letter not being preſerved, the purport of it may be inferred from the anſwer, which was as follows:

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* The perſons here referred to were both bred with him. His brother, Mr. ROGER HINDLEY, who has many years followed the ingenious profeſſion of a watch-cap-maker in London, was ſo much younger as to be an apprentice to him. Mr. JOHN SMITH, now dead, had ſome years paſt the honour to work in the inſtrument way, under the direction of the late Dr. DEMAINBRAY, for his preſent MAJESTY.

DEAR FRIEND, York, 13 March, 1748-9.

I THINK in your laſt you ſeem to be apprehenſive of ſome difficulties in drilling the hoop for dividing: Firſt, that the center of the hole in the hoop might not be preciſely in the center of the hole of the ſteel chaps, it was drilled in; but if I deſcribed fully to you the method I uſed, I can ſee no danger of error there: for my chaps were very thick, and the two correſponding holes were a little conical, and ground with a ſteel pin; firſt one pair, and then the other, alternately, till the pin would go the ſame depth into each. Then for drilling the hoop, I took any common drill that would paſs through, and bore the hole. After that, I took a five-ſided broach, which opened the hole in the braſs betwixt the ſteel chaps, but would not touch the ſteel; ſo conſequently the center of the holes in the braſs muſt be concentric with the holes in the chaps: and for alterations by air, heat, cold, &c. I was not above two or three hours in drilling a row of holes, as far as I remember.

2dly, For drilling, in a right line, I had a thin braſs plate, faſtened between the ſteel chaps, for the edge of the hoop to bear againſt, whilſt I thruſt it forward from hole to hole. What you propoſe of an iron frame with a lead outſide, will be better than my wooden block; but conſidering the little time that paſt, betwixt transferring the diviſions of the hoop to the diviſions of my dividing plate, I did not ſuffer much that way. It was when I drilled the holes in my dividing plate that I uſed a frame for drilling, which had one part of it that had a ſteel hole, that in lying upon the plane of the dividing plate was fixed faſt in its place for the point of the drill to paſs through: then, at the length of the drill, there was another piece of

ſteel,

ſteel, with a hole in it, to receive the other end of the drill to keep it at right-angles to the plane of the plate. This piece was a ſpring, which bended at the end, where it was faſtened to the frame of the lathe, at about eighteen inches from the end of the drill; ſo it puſhed the drill through with any given force the drill would bear: and though that end of the drill moved in the arch of a circle, it was a very ſmall part of it, being no more than equal to the thickneſs of the dividing plate.

My good wiſhes conclude me yours,

HEN. HINDLEY.

Whoever attentively conſiders the communication contained in the above letters will ſee, that more happy expedients could not have been deviſed to procure a ſet of diviſions, where there ſhould be the moſt exact equality among *neighbours*; and which, for the purpoſes of clock-making, is the principal thing to be wiſhed for. But herein, as in M. ROEMER's method, there were no means of checking the diſtant diviſions, which run on to 360: now ſuch a check, when the expanſion of metals is conſidered, and particularly the difference of expanſion between braſs and ſteel, ſeems abſolutely neceſſary for the purpoſe of diviſions upon inſtruments, where the accurate menſuration of large angles is required, as much as the equality of neighbouring diviſions *.

With this view the invention of this ingenious perſon ſuggeſted to him the thought of making his curved ſcrew to lay

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* The ingenious Mr. STANCLIFFE (ſome years a workman of HINDLEY's) has ſuggeſted, that the difference of expanſion between the ſteel chaps and the braſs hoop may be avoided by making the chaps of braſs alſo, with hard ſteel holes ſet ſeparately therein, ſomewhat ſimilar to the jewelled holes of watches.

hold

hold of fifteen teeth or degrees together: this, in effect, becomes a pair of compaſſes, 24 removes of which complete the whole circle, and produce 24 checks in the circumference: and whoever conſiders the very exquiſite degree of truth that reſults from the grinding of ſurfaces in contact, as already noticed, muſt expect a very great degree of rectification of whatever errors might ſubſiſt in the wheel after its firſt cutting.

What degree of truth it might in reality be capable of upon its firſt production and adjuſtment, is not now to be aſcertained, he never having uſed it for the graduation of any capital inſtrument. Thoſe that he made with a view to an accurate meaſure of angles, he always made with a ſcrew and wheel, or parts of circles cut by his engine into teeth, and ground together as before-mentioned; but I have reaſon to think, that its performance, if put to a ſtrict teſt, was never capable of that accuracy that he himſelf ſuppoſed it to have.

The method itſelf, however, from its ſimplicity, and eaſe of execution, ſeems to me to be a foundation for every thing that can be expected in truth of graduation; and in conſequence for reducing inſtruments to the leaſt ſize that is capable of bringing out all that can be expected from the largeſt; when it ſhall, like manual diviſion, have received thoſe advantages that the joint labours of the moſt ingenious men can beſtow upon it. That I may not appear to be without grounds for my expectations, I will beg leave to propoſe, what near forty years occaſional contemplation has ſuggeſted to me on the ſubject; and as I can deſcribe the proceſs I would purſue, where different from HINDLEY's, in fewer words than I could make out a regular criticiſm upon his letters, I will immediately proceed to the deſcription of it.

Propoſed Improvements *of* HINDLEY's *Method.*

I would recommend the number of parts into which the circle is to be reduced to be 1440; that is 4 times 360; which diviſions will therefore be quarters of a degree; the diſtances of the holes in the chaps will therefore, to a three-feet radius, be $\frac{157}{1000}$ of an inch nearly; that is, between the one-ſixth and one-ſeventh of an inch diſtance center and center.

Having provided myſelf with a ſtout mandrel, or arbor, for a *chock Lathe*, properly framed, that would turn a circle of ſix feet diameter, I would prepare a chock, or platform, for the end of it, of that diameter, or a little more, compoſed of clean-grained mahogany plank, all cut out of the ſame log; which, when finiſhed, to be about 1¼ inch thick, and formed in ſectors of circles, ſuppoſe 16 to make the circle; the middle line of each ſector lying in the direction of the grain of the wood, this will conſequently every where point outward: the method of framing this kind of work is well known.

The way of getting a ſlip of braſs to anſwer the circumference of this platform is ſuggeſted in Mr. BIRD's Account of conſtructing Mural Quadrants. Let a parallelogram of braſs of about three feet long, and of a competent ſubſtance (ſuppoſe half an inch) to make it when finiſhed about one-twentieth of an inch in thickneſs, be caſt of the fineſt braſs; and this to be rolled down till it becomes of ſufficient length for the hoop, and about one-fifth part more. I would then cut off, from the whole length, ſomewhat better than one-ſixth part, the whole being ſufficiently reduced to a thickneſs by the rollers. Perhaps no way will be more ready and convenient to get

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get ſuch a long ſtrip of braſs reduced to an equal breadth, than the method preſcribed by HINDLEY; viz. by turning it upon the chock prepared; but I would not make it wider on one ſide than the other, like the hoop of a caſk, as he deſcribes, but exactly to fit the chock, when truly cylindric; for the internal elaſticity of the braſs, in ſo great a length, will be very ſufficient for fitting it on tight enough, without any tapering. This I will now ſuppoſe done; and a pair of ſteel *chaps*, as deſcribed by HINDLEY, to be alſo prepared, and ready for grinding; which, by ſuch a careful admeaſurement as can eaſily be made, will give the length of the hoop ſufficiently near, on its firſt preparation.

Method of forming a Pair of Straps *as a* check *to the Diviſions.*

The part firſt cut off muſt be again cut into two equal parts in length; which, for diſtinction ſake, I will call the *ſtraps*; and which are to ſerve as checks to every 60th and every 120th diviſion of the circle.

A ſteel plate, of about half an inch in breadth, the ſame thickneſs as the ſtraps, and in length equal to the breadth of the hoop plate, muſt be ſoldered with ſilver ſolder to one end of each of the ſtraps, by which means their length will be increaſed half an inch by the ſteel. An hole muſt then be made through each ſteel plate, of the ſame ſize as thoſe through the chaps, and anſwerable to the middle of the ſtraps; but ſo near the border of the ſteel, that when the chaps are put on, and adapted to the ſteel hole, the next hole will fall through the braſs. The ſteel plates muſt then be hardened; and a pin being put through the two holes and the two plates, theſe muſt

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be wrought to a right line in contiguity to each other; by this means the ſtraight edge of each of the ſtraps will be reduced to the ſame diſtance from the ſteel hole: the hard ſteel edges may be rectified by the grindſtone, if neceſſary.

This being done, not only the holes in the chaps, but the holes in the two ſteel plates, applied to each other, like the two ſides of the chaps, muſt be reſpectively ground together; not with a taper pin, as preſcribed by HINDLEY; but ſo as not only to be cylindrical, but that the ſame cylindrical pin ſhall equally fit them all, and leave them ſmooth and poliſhed; which is a proceſs no ways difficult to a curious artiſt, and of which therefore a minute deſcription is unneceſſary.

The chaps being then put upon one of the ſtraps, with its ſtraight edge uppermoſt, and a pin put through the holes on the left-hand, and through the ſteel hole in the ſtrap under operation, the chaps muſt be ſet upright, ſo that the line joining the centers of the holes ſhall be parallel to the upper edge of the ſtrap; the braſs plate, mentioned by HINDLEY, between the chaps, as a guide for directing them always to that upright poſition, may be then adjuſted and fixed to the inſide of the chap next the operator *.

The performance of the enſuing part of this work ſhould be at a ſeaſon when the temper of the air is not very variable; rather above the mean temper (ſuppoſe at 60°) than below it;

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* It would be well, previous to the drilling of the ſteel chaps, that another hole was drilled in the chaps, that ſhould be ſomewhat above the upper edge of the ſtraps, and in the middle betwixt ſide and ſide, to receive a *ſteady pin* therein, antecedent to drilling the main holes; for then a tempered ſteel pin, a little taper, will, by driving it in as far as neceſſary, conſtantly anſwer this purpoſe from firſt to laſt, ſo as to regulate the holes in grinding, to be truly oppoſite: proper holes ſhould alſo be drilled for fixing the braſs guide plate to one of the chaps.

but

but above all things the artiſt ſhould be himſelf cool; that is, not in a ſtate of ſenſible perſpiration; and there ſhould be a free circulation of air in the room. Things being thus conditioned in reſpect to temperature, he may begin to drill the holes in one of the ſtraps; the pin being firſt put through the chaps and through the ſteel hole of the ſtrap; and the next hole, being drilled through the braſs with a common drill, that and every hole as it goes is to be finiſhed with a taper broach, as preſcribed by HINDLEY; and he may then prove or finiſh every hole by the application of a thorough broach, made ſo full as to require a degree of preſſure to force it through; and this broach being a little tempered, and the holes quite hard, there will be no fear of injuring the ſteel holes *.

Calling the hole in the ſteel plates 0, and obſerving the time of beginning, you may proceed to drill 60 holes as preſcribed by HINDLEY; and noting how long you have been about it, you may lay the work aſide a length of time, equal to the time you took in drilling; that any addition of warmth it may have acquired in handling or working may be again loſt in a great degree †. After this pauſe you may begin again, and go on to finiſh 60 holes more; that is, to the length of

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* The ſteel holes in the chaps need not to be above one-twentieth of an inch in diameter; and though it may be proper to make the ſteel plate, of which they are formed, one-tenth of an inch thick, in order to give the ſpring formed between them a convenient degree of ſtiffneſs, yet they may be reduced (by chamfering the outſides) to half that thickneſs.

† As there is not much occaſion for the artiſt to touch his work, the effects of that may alſo be very much avoided by wearing thick gloves; and the friction being but ſlight, and the work almoſt continually in the viſe, the variation of temperature in the metals concerned cannot be ſenſible or conſiderable.

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120 holes from the beginning; you then proceed in the ſame manner with the other ſtrap.

Method of drilling *the Hoop.*

You are now prepared to commence the work upon the long or *hoop-plate*; and you proceed therewith, in forming the firſt hole with the chaps, as before directed by HINDLEY, and this firſt hole you call o. You then place the ſtraps one on each ſide the hoop, with their gaged edges upward, and put the pin through the holes denominated 60 upon the ſtraps, and through the firſt hole already made, and denominated o upon the hoop; then, bringing the gaged edges of the ſteel plates to be even with the upper or working ſide of the hoop, you pinch them together in the viſe, and drill and broach the hole through the ſteel plates, which will make the hole, number 60, upon the hoop. This done, you put the pin through the left-hand hole of the chaps, and the hole marked o upon the hoop-plate firſt made, and proceed to drill with the chaps to 59 holes incluſive, which will fill up the whole ſpace from o to the 60th diviſion before obtained.

You now again have recourſe to the ſtraps, and placing them one on each ſide the hoop-plate, you put the pin through the 120th hole of the ſtraps, and through the hole marked o upon the hoop-plate; and regulating the ſteel plates to the hoop-plate as before, you drill and form a hole with the ſteel plates, which will correſpond with the 120th hole upon the hoop-plate; and afterwards filling up the 59 holes wanting, by means of the chaps, you then have all completed to the 120th diviſion, which is one-twelfth of the whole circle.

You then proceed, in like manner, with another ſet of 120 holes; that is, placing the 60th hole of the ſtraps to the 120th hole of the hoop-plate, and from it producing the 180th hole; you, in like manner as before, fill up this 60 with the chaps; and afterwards placing the 120th hole on the ſtraps in the 120th hole on the hoop-plate, you will obtain the 240th hole; ſo that filling up this laſt ſet of 60 diviſions, you have obtained 241 holes, including 240 ſpaces or diviſions of the hoop; and repeating this proceſs ten times more, you will, in like manner, obtain 1441 holes, comprehending 1440 ſpaces *. And this proceſs being carried on in temperate weather, the manner of working produces twelve ſimilar operations, wherein the materials and tools concerned will not only be ſubject to very little change of temperature, but that change, whatever it is, will be nearly ſimilar in each ſet of 120 holes: we may therefore infer, that the greateſt inequality, or indeed any that can be ſenſible, muſt be at every 60 diviſions, that is, between the 59th and 60th, and between the 119th and 120th, both which will be equally repeated 12 times, in the whole length which is to compoſe the *circumference of a Circle*, and which will thus be checked thereby 12 times in the circumference, and 12 times more at the intermediate diſtances; that is, with 12 maſter checks, and 12 ſubordinate ones, in the whole round.

It is proper here to obſerve, that in M. Roemer's method even ſixty diviſions could ſcarcely be truſted in an affair of great accuracy, on account of the objections already made, ariſing from the points having ſuch ſlight hold in the ſurface of the braſs; but here the parts are held ſo exceedingly firm, and the operation carried on with ſo much power, that any ſmall inequality in the hardneſs of the braſs, or irregularity of ſurface, cannot be ſuppoſed to affect the place of the center of the hole; nor will any ſmall inequality that may be ſuſpected from the wear of the ſteel holes ſenſibly affect the *center* of the hole, to which every thing is ultimately referred.

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* It will be proper, for reaſons hereafter to be mentioned, to continue the diviſions to 20 holes more, making in the whole 1461 holes.

Method of joining *the Hoop.*

A more happy thought than that of HINDLEY's, for joining the two ends of the hoop, could ſcarcely have been wiſhed for, in regard to preſerving the ſame equality of the ſpace between the holes contiguous to the joint, as in the other parts: for though, geometrically ſpeaking, the two *ſaddle* plates, in which the little cylindrical bolts are fixed, for bringing the terminating holes of the hoop plate to their due diſtance, being one applied within the hoop, and the other without, will belong to circles of different *radii*; yet this difference being exceedingly ſmall in ſuch thin metal, and ſo great a radius, and one being as much too big for the hoop as the other is too little, when the bolts are put in, and the hoop in that part ſet nearly to a circle by a mould; the mean between them aſſumed by the hoop, from the elaſtic compreſſibility of the materials, will be the truth.

It muſt, however, be remarked, that in the uſe of the ſtraps, the joining of the hoop ſhould not be made at any part betwixt an 119th and an 120th diviſion, as ſome inequality muſt be ſuppoſed there, unleſs the ſaddle plates were adapted thereto. The method the moſt eaſily practiſed, will be to continue the diviſion upon the hoop, about twenty more than the completion of the number intended to form the circle, and to cut off all the overplus ones at the beginning.

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The ſaddle plates I would recommend to contain ten holes each; ſo that if the diviſions are carried on to twenty more than what will be contained in the circle, there will be a piece containing twenty to cut off; and this again being cut in the middle will afford ten holes to make each ſaddle plate; ſo that there will be a place for a bolt on each ſide the joint, and then putting a bolt through every other hole, there will be three bolts at an end.

The pieces deſtined for the ſaddle plates, thus obtained, being broader than can be admitted when put to this uſe, I would adviſe to divide the breadth of the plate into three equal parts; and with a cutting hook (which perhaps will be attended with the leaſt violence in the ſeparation) to ſeparate the two outſide pieces from the middle piece: by this means the two ſaddle plates (though double) will occupy one third only of the breadth of the hoop in the middle; and two of the pieces cut off being applied, one on each ſide of the ſaddle plate on the outſide, will anſwer in like manner for the *rivet* plates.

The laſt operation to compleat the joining of the hoop is the putting on the rivet plates: to compleat this, I would adviſe a piece of braſs, of three or four inches in length, to be filed ſo as to anſwer to the inſide of the hoop, when reduced to a true circular form; and being three-eighths, or one-half an inch in thickneſs, to file the oppoſite ſide ſomewhat nearly concentric thereto; apply the middle of its convex arch to the inſide of the hoop at the joint, and then bringing on the middle of one of the rivet plates to the joint of the hoop, confine the three together by a couple of narrow-chapped hand viſes, leaving a ſpace between them capable of receiving a couple of pins as rivets on each ſide the joint; the holes for the rivets are then to be drilled through all, and a little ſmoothed with a broach at their entry, into which ſmooth taper pins are to be driven; not with violence, but moderately, that no ſenſible ſtretching of the ſolid parts may take place thereby; then cutting off and ſmoothing the heads, ſhift the viſes ſo as to receive another couple of holes, and a third couple in the ſame end of the hoop; and proceed progreſſively in the ſame manner, from the middle to the other end of the rivet plate; then gently ſeparate the internal braſs mould with a thin knife, or ſuch like inſtrument; and cutting off, and very lightly rivetting the inner ends, proceed to fix the other rivet plate, in the ſame manner, on the other ſide: by this means the hoop will be firmly joined in the very poſition given it by the ſaddle plates and mould. Theſe plates may then be removed, the inſide of the hoop cleared and ſmoothed, if neceſſary; and the outſide will have the middle part clear where the diviſions lie, and that without ſenſible loſs or gain in the juncture.

Method of transferring *the Diviſions of the Hoop to a dividing Plate.*

The hoop being thus refitted for the chock, that ſhould be turned down to leave a ſhoulder on one ſide, that the hoop, now reduced to an equal breadth, may be forced againſt it; and the diviſions, being equally diſtant from one of its edges, will be all found in a circle, as if turned upon it. It ſhould be very carefully fitted to the chock, that it may go on with a ſufficient degree of tightneſs, and without the neceſſity of much forcing; and it will be no inconvenience now, if it goes on upon a very ſlight degree of taper of the chock, as the internal ſpring of the materials will eaſily accommodate it to this ſhape without any injury to its general truth: a ſlight degree of a groove ſhould be turned in the place where the diviſions will come, that any conical pin, that is to ſerve as an index, let drop into the diviſions or holes, may not, by reaching through this thin plate, abut upon the wood, rather than upon the ſides of the holes: and thus this hoop is made into a wheel of 1440 equal diviſions, moveable round upon its own axis, whereon it was formed.

Againſt the time that this is compleated, there muſt be prepared a flat circular plate or wheel of braſs, the rim of which ſhould be of about 3½ inches breadth, and about two-tenths of an inch in thickneſs when finiſhed, to make a dividing plate; the external diameter of this is to be ſuch, that when laid flat upon the ſurface of the mahogany platform, its extreme edge will exceed the diameter of the hoop by about half an inch all round. There muſt alſo be prepared braſs arms (ſuppoſe eight in number) of an equal ſubſtance with the outer rim, and all connected with a circular plate in the middle; and, the whole of this work being framed beforehand, is to be let on flat upon the mahogany platform; whoſe face is ſuppoſed to be turned truly flat, and ſufficiently affixed with ſcrews: in this ſituation, the outward edge is to be turned, and the outward face of the rim turned flat. The *center plate*, which may be about twelve inches diameter, is alſo to be turned as flat as poſſible, and a center hole, of about half an inch diameter, to be very carefully turned therein.

A piece of clean, ſtraight-grained, well ſeaſoned mahogany, of about two feet long, three inches thick, and five or ſix inches broad, is then to be well affixed to ſome part of the general frame of the lathe, which muſt now have its poſition altered, ſo that the platform will become horizontal; and therefore the frame ſhould be originally made with this view [*]. The piece of mahogany is to be affixed ſo that one of its larger faces ſhall be in a parallel plane to the face of the platform, and ſo low as to clear the under ſide of the platform in its rotation; and ſo far diſtant from the center, that an index may be fixed upon this upper face of the piece of wood, ſo as conveniently to drop into the holes of the hoop; while the common *cutter frame* of a clock-maker's engine ſhall be firmly attached upon the ſame face of the wood, and ſo fixed as to cut the edge of the dividing plate into teeth, anſwerable to the ſeveral diviſions of the hoop. The teeth need only to be cut with a common cutter, making a parallel notch: and here it will be proper to obſerve, that not only both the index and cutter are to be founded on the ſame piece or baſe of wood; but that the nearer they are together, the more free they will be from the effects of all variations of expanſions by variations of temperature [†].

The equaliſing *the Teeth of the dividing Plate by* grinding.

The object of transferring the diviſions of the hoop to the teeth of the dividing plate, is ſtill farther to equaliſe the teeth by grinding; eſpecially thoſe that, falling within the compaſs of

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[*] After changing the poſition of the lathe, the collar of its mandrel ſhould be removed, and the neck made to move within three planes, ſo as to preſerve an exact center, in the manner of an *equal altitude* inſtrument.

[†] It is proper to obſerve, that as it may be impracticable to get the rim of the dividing plate caſt of the proper ſize, in one entire piece, it will be very practicable, if caſt of a leſs ſize (ſuppoſe half), but of a ſufficient thickneſs, to roll it down; and by having the outward edge originally thicker than the inner, in the proportion of the *radii*, it may be ſo managed by the rollers as to be of an equal thickneſs when brought to its proper ſize. But the arms and center plate ſhould be of the ſame metal, rolled in the ſame degree.

each ſet of 120 diviſions, may be ſuppoſed, if any, to be mended thereby; but as it may be incommodious to conſtruct a curved ſcrew, of ſuch a length and ſize, in HINDLEY's method, as would be ſufficient for the purpoſe, I would propoſe to uſe two ſcrews of braſs, cut from a cylinder in the way ſet forth by Mr. RAMSDEN, each of which, with a very little grinding upon this large circumference, would lay hold of ten or twelve teeth together. I would place the two ſcrews, that is, their middles, to be ninety diviſions aſunder; of conſequence, when one of the ſcrews is between the 59th and the 60th, or between the 119th and 120th diviſion of each ſet, the other will be in the middle of the ſpace divided by the chaps only*.

The threads of theſe ſcrews I would adviſe to be cut a little taper, ſo that as they grind in, they may fill the notches of the teeth; which alſo, by this means, will acquire a little tapering towards their extremities; and by cutting the notches parallel, as I have mentioned, the true ground part will always be certain of being at the extremity.

When the ſcrews have been uſed in grinding till they are found to have the effect of a perfectly equal and eaſy rotation all round, and all the teeth reduced to a ſenſible taper, and regular bearing, I would then totally remove the ſcrews from the ſquare block of wood, upon whoſe upper face I ſuppoſe them to have been mounted; in like manner as I ſuppoſe the index and cutting frame to have been removed, to make room for the mounting of the ſcrews. I now conſider the teeth of the dividing plate, ſo formed, as having all the equality that the preſent known ſtate of human art has pointed out; and the whole convertible upon the axis or mandrel upon which it has been originally formed, and the central hole of the plate concentric therewith: I therefore conſider the ground faces of the teeth of the plate as the actual diviſions. It now remains to ſhew how they are to be transferred, to form the diviſions of an inſtrument.

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* The beſt way of giving an equal motion to thoſe two ſcrews, ſeems to be by a detached axis, carrying two common flat wheels; one acting upon a like flat wheel, upon the axis of one ſcrew, and the other, in the ſame manner, upon the other; and applying the pulley for communicating the power to the middle of the detached axis between the two wheels, the ſpring or twiſt will be equal both ways; ſo that in turning the contrary way round, they will ſtill be in equal advance.

Preparation *of the dividing Plate for* graduating *Inſtruments.*

If a ſmall cylinder of hard ſteel is duly poliſhed, and made of a ſize ſo as juſt to chock in betwixt the extremities of the teeth, then the center of that cylinder will be a fixed point, in reſpect to the circumference of the wheel: if another cylinder is applied in like manner, at the diſtance of a number of diviſions (ſuppoſe it a prime number, ſo as to croſs all former diviſions, viz. 17 or 19), then the middle of the line joining the centers of the two cylinders will remain in the direction of the *ſame radius*, though one of them ſhould force in a minute quantity further than the other; and if a point is aſſumed in the direction of a tangent to a circle at this middle-point, then though both the cylinders ſhould drop in a minute quantity further at one time than another, yet the middle-point would remain at the ſame diſtance from the point in the tangent; provided that point was removed to a competent diſtance, that is, to five or ſix inches. On this principle I would conſtruct an index, the two cylinders being fixed in a frame, convertible about the middle-point, and to be centered in the end of the lever, repreſenting the tangent; then this lever being again convertible about the point in the tangent line, the middle-point would always have a fixed diſtance

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from the point in the tangent, and there hold it ſteadily faſt; the tangent point being placed upon the fixed block before-mentioned.

Uſe *of the dividing Plate in the* Graduation *of Inſtruments.*

Our dividing plate is now ready for the reception of an inſtrument; ſuppoſe it a quadrant, whoſe radius, however, muſt not exceed the radius of the dividing plate: It is to be laid upon the face of the dividing plate, and a weight, or weights, equivalent to that of the quadrant, is placed on the oppoſite ſide, to balance it. It muſt alſo be ſuppoſed, that the quadrant is made with a view to be divided by this engine; and conſequently, that the central cylinder is ſo well adapted, and nicely fitted to the center hole of the quadrant, that the center cylinder can be removed, in order for the limb to be divided, and again replaced, without ſenſibly altering its center. This being the caſe, let a piece of metal be turned, to apply to the quadrant, perfectly like its center cylinder at the upper end, and turned nicely to fit the central hole in the dividing plate, at the lower end; then, the quadrant being fixed with proper faſtening ſcrews, I would cut the diviſions with a beam compaſs; and, if a fixed point is aſſumed, *viz.* the center of the tangent point for the index; then the beam compaſs being always opened to the computed length of the tangent of the circle of diviſions, it will be ſufficiently near for cutting the diviſions, ſquare to the circular arches between which they are placed.

It will alſo be proper (to prevent unequal expanſions) that the beam of the compaſs ſhould be formed of a piece of clean-grained *white fir*; and that the length between the points be incloſed in a tube of tin or braſs; without touching the beam, except at the terminations, which will in a great meaſure protect it from both alteration of moiſture, and of heat from the body of the artiſt, during the operation.

It will be likewiſe proper to have a lever, or ſome equivalent contrivance, to bring the dividing plate forward; that after lifting the little cylinders out of the diviſions, and reſting them upon the tops of the teeth, they may be brought gently forward with an equal drag, and ultimately ſnap in between the teeth, by the ſtrength of the ſpring commanding the index; by this means the drag of the friction of the whole will be conſtantly the ſame way.

Concluſion.

Now, if, as it has been ſhewn, a quadrant of any radius may be read off to the 4000dth part of an inch, then this quantity upon a radius of three feet will not be ſo much as 1½ ſecond; and as the whole of the proceſs is carried on by contact, in which a greater error than that of a 60,000dth part of an inch cannot be admitted in any ſingle operation, I ſhould aſſuredly expect a three-feet quadrant, ſo divided, to be true in its diviſions, and read off to at moſt two ſeconds.

But, after all, in an inſtrument like this, I ſhould expect the greateſt ſource of error to be in the want of perfect coincidence of the center of the diviſions with the actual center upon which the index revolves; and therefore, that if, inſtead of a quadrant of three-feet radius, a complete circle of five feet diameter was divided, and its diviſions read off from the two oppoſite points (taking the mean), then the errors of the center will be wholly avoided. For this reaſon, I am very clearly of opinion, that the ſagacious propoſition of Mr. RAMSDEN,

DEN, to uſe circles inſtead of quadrants, or other portions of circles, will bid much the faireſt for perfection in actual practice; and that his ingenious method of making them both ſtiff and light, by the uſe of hollow conical tubes by way of ſpokes, in the manner of a common wheel, will enable him to mount them of five feet diameter, upon hollow axes, in the nature of a *tranſit*. By this means we ſhall have all the good properties of both the quadrant and tranſit united in one inſtrument; and obſervations both of right aſcenſion and declination, through the very ſame teleſcope, as long ſince attempted by M. ROEMER; and to a degree of perfection and certainty, in point of declination, hitherto unattainable by the largeſt inſtruments that have yet been made.

N. B. In matters of very nice determination, ſmall circumſtances often come to be of conſequence; and it is in this view that I mention what follows. It was a practice of HINDLEY's of many years ſtanding, and ſince followed by myſelf and others, wherever he made any uſe of the *vernier*, to lay the vernier plate in the ſame plane, or cylindrical ſurface continued, whereon the principal diviſions are cut. It is of equal utility, though the vernier be rejected, to lay the index ſtroke in the plane of the diviſions. In this way the diviſions being by convenience upon the external border of the limb[*], two ſets of diviſions are thereby rendered incommodious; but thoſe that wiſh two ſets, as a check, will in a great meaſure aid themſelves, by reading from two different parts of the ſame ſet of diviſions; which is very eaſily provided for, by putting an additional ſtroke upon the index plate, at the diſtance of 9, 11, or any prime number of diviſions to 19, 23, or more; and reading off from that ſtroke alſo; as before recommended for great quadrants, where the vernier is propoſed to be rejected *: ſo that they will thereby be mutually checked by diviſions that had no correſpondence in their original formation.

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[*] It has been objected, that laying the diviſions upon the extreme edge of the limb of the inſtrument ſubjects it to injury: but, to obviate this, in an HADLEY's quadrant made for me, by my direction, by the late Mr. MORGAN, in the year 1756, wherein the vernier is laid even with the diviſions, thoſe are protected by a projection of the ſolid part of the limb, beyond the diviſions; a *Rabbet* being ſunk in the edge of the limb, to clear the vernier.

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* I would not have it thought, from my propoſal of rejecting the vernier, that I have any quarrel with it; I think it a very ſimple and ingenious contrivance, where it is properly applicable; that is, where the ſtrokes of the vernier, or their eſtimated halves, are ſufficient for all the preciſion required or expected from the inſtrument, as in Hadley's quadrants, theodolites, &c.: but where ſtill more minute diviſions are required than can eaſily be had by eſtimation from the vernier; to do this by a ſcrew, as a *ſupplement* to the vernier, appears to me in the light of bringing a more accurate tool to ſupply the deficiencies of one leſs accurate; when the former might, with more propriety, ſupply the place of the latter altogether.

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II. *A Series of Obſervations on, and a Diſcovery of, the Period of the Variation of the Light of the Star marked* δ *by* Bayer, *near the Head of* Cepheus. *In a Letter from* John Goodricke, *Eſq. to* Nevil Maſkelyne, *D. D. F. R. S. and Aſtronomer Royal.*

Read November 24, 1785.

SIR, York, June 28, 1785.

THE improvements which of late years have been introduced into aſtronomy, ſhould be attributed not only to the diligence and accuracy wherewith aſtronomers proſecute their obſervations and diſcoveries, but in part alſo to your exertions, and eſpecially to that kind encouragement which you have, on many occaſions, afforded thoſe who make this ſcience their chief ſtudy; and I am happy to have this opportunity of acknowledging myſelf one of thoſe who are much indebted to you in this reſpect. Under theſe impreſſions I thought I could not do better than to addreſs to you the following account of a periodical variation in the ſtar δ Cephei, which I lately diſcovered. This account will, I preſume, be a conſiderable addition to the few diſcoveries that have but very lately been made reſpecting the ſame ſubject. They may probably lead to ſome better knowledge of the fixed ſtars, eſpecially of their conſtitution and the cauſe of their remarkable changes.

My firſt obſervation was Oct. 19, 1784; and as I wiſhed to eſtabliſh the ſeveral points of the variation with as great accuracy as the nature of the ſubject will admit of, I have delayed ſending this account till now; but as obſervations made through ſo long an interval of time muſt be very numerous, and would only ſwell this paper to an unneceſſary length, I have in the following ſeries formed a ſelection, chiefly of thoſe that were made under the moſt favourable circumſtances; and I muſt add, that none of thoſe that are omitted contradict the reſults. From this ſeries I have ſettled, that the ſtar has a periodical variation of 5 d. 8 h. 37′½, during which time it undergoes the following changes:

1. It is at its greateſt brightneſs about one day and thirteen hours.

2. Its diminution is performed in about one day and eighteen hours.

3. It is at its greateſt obſcuration about one day and twelve hours.

4. It increaſes in about thirteen hours.

When it is in the firſt point it appears as a ſtar of between the fourth and third magnitude; but its relative brightneſs does not ſeem always to be quite the ſame, being ſometimes between ζ and ι Cephei, and ſometimes only equal to, or ſomething leſs than, ι Cephei, or between ζ Cephei and 7 Lacertæ. In the third point it appears as a ſtar of between the fourth and fifth magnitude, if not nearer the fifth; and its relative brightneſs is as follows: *nearly* equal to ε and ξ Cephei, and conſiderably leſs than 7 Lacertæ.

The relative brightneſs and magnitude of thoſe ſtars to which the variable one was compared, is as follows: ζ Cephei, the brighteſt, is between the third and fourth magnitude; ι Cephei, the next brighteſt, is between the fourth and third; 7 Lacertæ is leſs than ι Cephei, and of about the fourth magnitude; ε Cephei is between the fourth and fifth magnitude; and ξ Cephei, which is a little leſs than ε, is between the fifth and fourth.

A Series of Obſervations on the Variation of the Light of the Star δ Cephei.

1784, Oct. 19, at 8½ h. I thought it was rather leſs than ζ Cephei.

Oct. 20, at 8½ h. it was rather leſs than ζ, and about equal to ι Cephei.

Oct. 22, at 9½ h. leſs than ι, and larger than ε Cephei; but the air was not very favourable.

Oct. 23, at 6½ h. and 11 h. leſs than 7 Lacertæ, and a little brighter than ε Cephei.

Oct. 24, at 6½ h. leſs than ζ Cephei, ſomewhat leſs than ι Cephei, and ſomething brighter than 7 Lacertæ; ſtrong moon-light, and air rather hazy.

At 8½ h. to 11 h. a little leſs than ζ and ι Cephei, and brighter than 7 Lacertæ; air clear and froſty; the moon was very low at 11 h.

Oct. 25, at 6 h. 8 h. and 11 h. nearly the ſame; air pretty clear, and moon bright.

Oct. 26, at 9½ h. and 11 h. rather leſs than 7 Lacertæ; ſtrong moon-light, but air very clear.

Oct. 27, 6½ h. and 10½ h. leſs than 7 Lacertæ, and brighter than ε Cephei; ditto.

Oct. 28, at 9½ h. and 12 h. juſt the ſame, if not leſs; moon-light, but the air was remarkably clear.

Oct. 31, at 8 h. nearly equal to, if not leſs than, 7 Lacertæ.

Nov. 1, at 11½ h. ſomewhat leſs than 7 Lacertæ; air clear.

Nov. 3, at 12½ h. equal to, if not a little leſs than, 7 Lacertæ; but the weather was not very favourable: it ſeemed to have increaſed ſince my firſt obſervation, which was at 5½ h.

Nov. 5, at 13 h. brighter than 7 Lacertæ, and leſs than ζ Cephei; flying clouds, but air pretty clear.

Nov. 6, at 9 h. and 12½ h. rather leſs than 7 Lacertæ.

Nov. 7, at 7½ h. I thought it ſtill rather leſs than 7 Lacertæ, but at 10½ h. and 11 h. it was evidently leſs than it; air clear.

Nov. 10, at 11 h. and 12½ h. ſomething leſs than ξ Cephei, and brighter than 7 Lacertæ; clear ſky.

Nov. 11, at 7 h. to 12 h. a little brighter than 7 Lacertæ.

Nov. 12, at 7 h. and 8½ h. about equal to 7 Lacertæ. From 9½ h. to 12½ h. it was ſomething leſs than 7 Lacertæ, and brighter than ε Cephei.

Nov. 13, at 6½ h. to 11 h. only a little brighter than ε Cephei, though ſometimes it appeared equal to it.

Nov. 14, at 7½ h. brighter than ε Cephei, and, I believe, equal to 7 Lacertæ. There was a hazineſs about 7 Lacertæ.

Nov. 15, at 12 h. leſs than ζ Cephei, and brighter than 7 Lacertæ; fine *aurora borealis*, but the air was very clear.

At 18½ h. ditto; but the air was not very clear.

Nov. 16, at 6½ h. and 10 h. juſt the ſame, if not decreaſed at 10 h.

Nov. 17, at 6½ h. to 10½ h. a little leſs than 7 Lacertæ, and brighter than ε Cephei; air clear.

Nov. 18, at 9 h. to 12 h. and 19 h. little brighter than ε and ξ Cephei.

Nov. 19, at 6 h. to 10 h. juſt the ſame, being but a very little brighter than ε and ξ Cephei; air clear.

At 18½ h. it was increaſed, being now brighter than ε and ξ Cephei.

Nov. 20, 7 h. to 11 h. conſiderably brighter than 7 Lacertæ, ſomething leſs than ζ Cephei; air extremely clear at 11 h.

Nov. 21, at 6 h. exactly the ſame.

Nov. 22, at 9¼ h. about equal to 7 Lacertæ; moon-light.

Nov. 25, at 7 h. and 8 h. leſs than 7 Lacertæ, and brighter than ε and ξ Cephei; air clear.

At 9½ h. and 9¾ h. a little brighter than 7 Lacertæ.

At 10½ h. and 12 h. brighter than 7 Lacertæ, and about between ζ Cephei and 7 Lacertæ, but rather nearer 7 Lacertæ; air clear and moon-light.

Nov. 26, at 9 h. exactly as laſt night.

Nov. 29, at 7½ and 8 h. leſs than 7 Lacertæ, and ſomething brighter than ε and ξ Cephei.

Nov. 30, at 8¼ h. as laſt night; air clear.

At 10¼ h. between 7 Lacertæ and ε Cephei, but nearer ε.

At 10½ h. 11 h. and 12 h. ditto, but nearer 7 Lacertæ; air clear. I have no doubt of its increaſe ſince 8¼ h. Mr. E. PIGOTT found it rather leſs than ζ Cephei at 18½ h. *See his Obſervations.*

Dec. 1, 11 h. ſomething leſs than ζ Cephei, and brighter than 7 Lacertæ.

Dec. 3, 12¼ h. leſs than 7 Lacertæ, and brighter than ε Cephei.

Dec. 4, 5½ h. to 12 h. little brighter than ε and ξ Cephei.

Dec. 7, 10 h. and 11 h. between ζ Cephei and 7 Lacertæ.

Dec. 8, at 10½ h. between 7 Lacertæ and ε Cephei.

Dec. 9, 11½ h. ditto, but nearer ε Cephei; about equal to ξ Cephei.

Dec. 11, 6 h. ſomething leſs than 7 Lacertæ; brighter than ε Cephei.

At 7½ h. ſomething brighter than 7 Lacertæ.

At 8½ h. brighter than 7 Lacertæ.

At 9 h. and 11 h. between ζ Cephei and 7 Lacertæ, but nearer 7 Lacertæ.

Dec. 12, at 6 h. ſomething leſs than ζ Cephei.

Dec. 13, at 9½ h. brighter than 7 Lacertæ; conſiderably leſs than ζ Cephei.

Dec. 14, at 8½ h. nearly equal to, if not leſs than, 7 Lacertæ.

Dec. 17, at 5½ h. and 7½ h. equal to, if not leſs than, ι Cephei, and between ζ Cephei and 7 Lacertæ, but nearer ζ.

Dec. 18, at 9 h. leſs than ι Cephei, and between ζ Cephei and 7 Lacertæ, but nearer 7 Lacertæ.

Dec. 19, at 19 h. leſs than 7 Lacertæ; conſiderably brighter than ε and ξ Cephei.

Dec. 20, at 6 h. and 7 h. about equal to ξ Cephei, and a little brighter than ε Cephei.

Dec. 21, at 8 h. and 18 h. nearly equal to ι Cephei.

Dec. 22, at 8½ h. conſiderably brighter than 7 Lacertæ, leſs than ζ, and a little leſs than ι Cephei; ſtrong moon-light.

Dec. 25, at 5½ h. between 7 Lacertæ and ε Cephei.

Dec. 28, at 8 h. &c. between ζ Cephei and 7 Lacertæ, and equal to, if not leſs than, ι Cephei.

Having, in the beginning of this paper, mentioned my intention of omitting ſeveral obſervations, in order to be as ſhort as poſſible, I have thought it beſt, with the exception of one only, to leave out all that were made in January, February, and March, becauſe they were much interrupted by the then unfavourable ſtate of the weather.

1785, Feb. 8, at 9 h. equal to 7 Lacertæ; conſiderably leſs than ι Cephei.

At 10 h. rather brighter than 7 Lacertæ.

At 11 h. brighter than 7 Lacertæ; a little leſs than ι Cephei.

April 1, at 11 h. about equal to ε and ζ Cephei; weather not favourable.

April 2, at 12½ h. ditto.

April 3, at 8 h. a little leſs than ι Cephei, leſs than ζ Cephei, and brighter than 7 Lacertæ.

April 4, at 12 h. ditto; if any thing, it is leſs than it was laſt night.

April 7, at 10 h. about equal to ε and ζ Cephei; but the weather was not very favourable.

April 8, at 7½ h. conſiderably leſs than ι Cephei, brighter than ε and ζ Cephei; but the air was not very clear.

At 10 h. it was increaſed.

At 11 h. only a little leſs than ι Cephei.

At 12 h. equal to, if not a little brighter than ι, and leſs than ζ Cephei; conſiderably brighter than 7 Lacertæ.

April 12, at 12 h. a little leſs than ε, and nearly equal to ζ Cephei.

April 13, at 9 h. juſt the ſame.

At 11 h. ſeemed rather increaſed, being equal to ε, and a little brighter than ζ Cephei.

April 16, at 11½ h. nearly equal to ε and ζ Cephei.

April 17, at 9 h. and 11 h. rather a little leſs than ε, and a little brighter than ζ Cephei.

April 19, at 11½ h. about equal to ι Cephei, if not a little brighter than it; leſs than ζ Cephei, and conſiderably brighter than 7 Lacertæ.

April 24, at 10 h. a little brighter than 7 Lacertæ; conſiderably leſs than ι Cephei.

At 12 h. ſcarce at all altered, but if any thing it is a little increaſed; air very clear, and obſervation good.

April 25, at 10 h. and 11½ h. little leſs than ι Cephei, and conſiderably brighter than 7 Lacertæ.

April 26, at 10 h. and 11 h. leſs than 7 Lacertæ, ſomething brighter than ε, and brighter than ξ Cephei.

May 4, at 9½ h. and 12 h. a little leſs than ε and ξ Cephei.

May 7, at 12 h. leſs than ι Cephei, and a little brighter than 7 Lacertæ.

May 9, at 11 h. a little leſs than ε Cephei.

May 10, at 12 h. between 7 Lacertæ and ε Cephei, but ſomething nearer ε.

May 11, at 10 h. and 12 h. brighter than ι Cephei, leſs than ζ Cephei, and much brighter than 7 Lacertæ.

May 14, at 11½ h. much leſs than 7 Lacertæ, equal to, if not a little brighter than, ε Cephei, and brighter than ξ Cephei.

May 15, at 9½ h. leſs than ε, and about equal to ξ Cephei.

May 19, at 9½ h. and 11 h. equal to, if not a little brighter than, ε Cephei, and brighter than ξ Cephei.

May 20, at 9½ h. 11 h. and 12 h. a little leſs than ε, and nearly equal to ξ Cephei.

May 21, at 12 h. equal to, if not a little leſs than, ι Cephei; leſs than ζ Cephei, and conſiderably brighter than 7 Lacertæ.

May 22, at 10 h. and 11½ h. a little brighter than ι Cephei, the reſt as laſt night.

May 23, at 11 h. and 11½ h. nearly equal to 7 Lacertæ, and leſs than ι Cephei.

May

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May 25, at 10 h. and 12 h. a little leſs than ε, and about equal to ξ Cephei.

May 27, at 10 h. between ζ and ι Cephei, and conſiderably brighter than 7 Lacertæ.

May 28, at 12 h. between ι Cephei and 7 Lacertæ.

June 1, at 9½ h. I thought it leſs than ι Cephei; air not clear, and twilight pretty ſtrong.

At 10½ h. and 12 h. between ζ and ι Cephei, but rather nearer ι.

June 2, at 12 h. exactly the ſame.

June 6, at 12 h. ditto; the weather was not very favourable, but the obſervation ſeemed good.

June 10, at 11½ h. a little leſs than ε Cephei.

June 12, at 11 h. between ζ and ι Cephei.

June 21, at 10 h. nearly equal to, if not a very little brighter than, ε Cephei; twilight.

At 11½ h. a little leſs than ε, and about equal to ξ Cephei.

June 23, at 11½ h. between ζ and ι Cephei, and brighter than 7 Lacertæ.

June 24, at 11½ h. ditto; only a ſhort view.

June 25, at 11½ h. a little, but certainly, brighter than ε Cephei, brighter than ξ Cephei, and conſiderably leſs than 7 Lacertæ.

June 26, at 11½ h. a little leſs than ε Cephei, and equal to, if not a little brighter than, ξ Cephei.

In the above collection I find only two or three miſtakes of any conſequence, *viz.* the dates of the obſervations of April 7, and 8, are marked in my journal for April 8, and 9; but I have corrected them, being convinced they are erroneous: and the obſervation of May 10, I think, diſagrees rather too much from what it ought to be by computation.

The

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The following obſervations were made by my friend Mr. E. Pigott; who, at my requeſt, was ſo kind as to obſerve the ſtar as often as poſſible, though then in an ill ſtate of health. They are, I preſume, ſufficient to corroborate the variation of the ſtar as above ſtated, although in one or two places there may be found ſome little differences between our obſervations.

MR. PIGOTT'S OBSERVATIONS.

1784, Oct. 25, at 12 h. rather brighter than 7 Lacertæ; much brighter than ε Cephei, and much leſs than ζ Cephei; nearly between ζ Cephei and 7 Lacertæ.

Oct. 26, at 12 h. ſeemed the ſame as yeſterday.

Nov. 1, at 12 h. brighter than ε Cephei; ſeemed rather leſs than 7 Lacertæ.

Nov. 13, at 8¾ h. rather, but very little, brighter than ε Cephei; leſs than 7 Lacertæ.

Nov. 15, at 12 h. ſeemed rather brighter than 7 Lacertæ, and leſs than ζ Cephei.

Nov. 17, at 8 h. leſs than 7 Lacertæ; rather brighter than ε Cephei.

Nov. 18, at 12 h. equal to ε Cephei, though ſometimes it ſeemed leſs; leſs than 7 Lacertæ.

Nov. 19, at 12 h. ſeemed equal to ε Cephei.

Nov. 20, at 11 h. rather leſs than ζ Cephei; brighter than 7 Lacertæ.

Nov. 25, at 11½ h. if not equal rather brighter than 7 Lacertæ; much brighter than ε Cephei.

Nov. 29, at 8 h. equal to ε Cephei.

Nov. 30, at 11¾ h. brighter than ε Cephei; leſs than 7 Lacertæ.

At 18½ h. much increaſed; rather leſs than ζ Cephei.

Dec. 4, at 6½ h. ſometimes thought it leſs, and at other times brighter, than ε Cephei.

Dec. 11, at 5¾ h. leſs than 7 Lacertæ; rather brighter than ε Cephei.

At 11½ h. rather brighter than 7 Lacertæ; not at its full brightneſs.

Dec. 21, at 7 h. if any difference leſs than ε Cephei.

At 18¾ h. a little brighter than ε Cephei.

Dec. 22, at 8 h. leſs than ζ Cephei; a little brighter than 7 Lacertæ.

Dec. 28, at 5¾ h. nearly equal to ζ Cephei; had only a ſhort view of them.

1785, April 26, at 11½ h. leſs than ζ, rather leſs than ι Cephei, brighter than ε Cephei, and if any difference rather brighter than 7 Lacertæ.

May 4, at 9¾ h. much leſs than ζ Cephei, leſs than ι Cephei, and than 7 Lacertæ, and rather brighter than ε Cephei.

May 7, at 11 h. rather leſs than ι Cephei, and brighter than ε Cephei.

May 9, at 11¾ h. rather brighter than ε Cephei, and much leſs than ι Cephei, and 7 Lacertæ.

May 11, at 10½ h. rather leſs than ζ, and rather brighter than ι Cephei; much brighter than 7 Lacertæ.

May 19, at 10 h. equal to ε Cephei, but if any difference rather brighter; little hazy and moon-light. The ſame at 12 h. but the weather was not hazy then.

May 20, at 11¾ h. and 12¾ h. rather brighter than ε Cephei, and much leſs than 7 Lacertæ; moon-light ſtrong at 11¾ h.

May 21, at 12¾ h. equal to 7 Lacertæ; leſs than ι Cephei.

May

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May 22, at 12¼ h. equal to, if not brighter than, 7 Lacertæ; think it brighter than ι Cephei.

May 23, at 11¼ h. ſeemed ſometimes equal to, though generally leſs than, ι Cephei and 7 Lacertæ.

Having now delivered the obſervations, from whence I have deduced the preceding concluſions, nothing more relative to this ſubject remains to be mentioned, except the determination of the period; in the doing of which I muſt follow nearly the ſame methods as have been uſed in ſome preceding papers. It is very evident, from a rough calculation, where only ſingle periods or very ſhort intervals are uſed, that it is about five days and eight hours. In order to determine this period with greater exactneſs, I have, in the following table, collated ſome of the moſt preciſe phaſes. The firſt five are times when δ Cephei was obſerved to be equal to 7 Lacertæ during the courſe of its increaſe of brightneſs, which proceeds rapidly. The five next are ſimilar times, with this only difference, that as it was not then actually obſerved to be equal to 7 Lacertæ, a proper allowance from the neareſt obſervations was made on ſuppoſition that the changes are ſimilar in every period. The ten laſt are aſſumed times between its leaſt and greateſt brightneſs, which determinations can hardly err more than a few hours, as the whole increaſe is completed in thirteen hours; but even were it ſo, the periods deduced from them would ſtill be exact, becauſe the intervals are very long.

1784 and 1785.

D.	H.		D. H. M.
Nov. 25, Feb. 8,	8¼ 9	an interval of 14 periods each of	5 8 34+
Dec. 11, Feb. 8,	6¾ 9	ditto 11 ditto	5 8 54¼
Nov. 25, April 8,	8¼ 8	ditto 25 ditto	5 8 36
Nov. 30, April 8,	16 8	ditto 24 ditto	5 8 40
Nov. 30, April 24,	16 8	ditto 27 ditto	5 8 35½−
Oct. 23, May 21,	21 0	ditto 39 ditto	5 8 41½+
Nov. 19, May 21,	22 0	ditto 34 ditto	5 8 31¼
Dec. 22, May 21,	1 0	ditto 28 ditto	5 8 32 +
Nov. 19, April 2,	22 23	ditto 25 ditto	5 8 40¾
Nov. 19, Dec. 22,	22 1	ditto 6 ditto	5 8 30

Hence the period is, on a mean, - 5 8 37½+

A few curſory remarks ſhall conclude this Paper. What I have before mentioned, that the greateſt brightneſs of δ Cephei does not ſeem to be always quite the ſame, is not peculiar to this ſtar, but is alſo to be obſerved in the other variable ones. I have remarked in a late Paper, that the greateſt brightneſs of β Lyræ is ſubject to conſiderable alterations, and thought then that it might be owing to ſome fallacy of obſervation; but now

I have reaſon to alter, in ſome meaſure, my opinion on this head. Even Algol does not ſeem to be always obſcured in the ſame degree, being perceived to be ſometimes a little brighter than ρ Perſei, and ſometimes leſs than it [*]. Theſe ſeeming irregularities, however, do not appear to affect the period; for if we compare the ſame preciſe phaſes together, it will be found ſtill regular. This may, I ſuppoſe, be accounted for, by a rotation of the ſtar on its axis, having fixed ſpots that vary only in their ſize.

I need not ſay, that the ſituation of δ Cephei, on account of its great northern declination is ſuch, that its changes may be obſerved with great advantage in theſe latitudes, it being always ſufficiently elevated above the horizon. To this circumſtance are alſo owing its various changes of poſition, which, I find, affect the comparative brightneſs of the ſtars a little; but, as theſe differences are very trifling, I ſhall take no further notice of them.

If you think this account worthy of notice, I beg you will be ſo kind as to communicate it to the Royal Society.

I remain, with great regard, &c.

JOHN GOODRICKE.

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[*] This will appear from an attentive examination of the obſervations of that ſtar's diminution in my two late Papers, which were printed in the LXXIIId and LXXIVth volumes of the Philoſophical Tranſactions. I did not take much notice of it then, becauſe I thought the difference was too ſmall to be relied on; but the obſervations I have made ſince ſeem to confirm that it does *really* diminiſh a little unequally. M. MECHAIN, in a letter to Mr. E. PIGOTT, mentions the ſame fact.

III. *Magnetical Experiments and Obſervations.*

By Mr. Tiberius Cavallo, *F. R. S.*

(The Lecture founded by the late HENRY BAKER, Eſq. F.R.S.)

Read November 24, 1785.

THE object of this lecture is to ſhew the properties of ſome metallic ſubſtances with reſpect to magnetiſm; and the experiments herein related ſeem to aſcertain ſome new and remarkable facts.

The magnetic properties have been generally thought to belong only to iron, or to thoſe ſubſtances which contained that metal; comprehending under the general name of iron not only the metal commonly ſo called, but likewiſe its more perfect and more imperfect ſtates, *viz.* ſteel, iron ores, amongſt which is conſidered the magnet, and the calces of iron, excepting only thoſe which are very much dephlogiſticated, for they poſſeſs no magnetic property whatſoever. Some other metallic ſubſtances, and eſpecially platina, braſs, and nickel, on which the magnet has ſome action, were thought to be magnetic ſo far as they contained ſome portion of iron, the preſence of which may be manifeſted by chemical methods in many caſes, but not always; becauſe the quantity of iron may be ſo exceſſively ſmall in proportion to the weight of the other metal in which it is concealed, as not to be diſcoverable by chemical analyſis, and yet it may be ſufficient to affect the magnetic needle. The following experiment will ſhew, that an exceedingly ſmall quantity of iron will render a body ſenſibly magnetic.

Having choſen a piece of Turkey-ſtone, which weighed about an ounce, I examined it by a very ſenſible magnetic needle, and found that it had not the leaſt degree of magnetiſm, the needle not being moved from its uſual direction by the vicinity of any part of the ſurface of the ſtone; I then weighed a piece of ſteel with a pair of ſcales that turned with the twentieth part of a grain, and afterwards drew one end of it over the ſurface of the ſtone in various directions. This done, the piece of ſteel was weighed again, and was found to have loſt ſo ſmall a part of its weight as not to be diſcernible by that pair of ſcales; yet the Turkey-ſtone, which had acquired only that ſmall quantity of ſteel, affected the magnetic needle very ſenſibly. Chemiſtry ſeems not to afford any means by which ſo ſmall a quantity of iron may be deciſively detected in a body that weighs one ounce. Hence it follows, that though no iron is to be diſcovered in a body by chemical methods, yet it ſhould not be concluded, that the ſaid body, if it affect the magnetic needle, does not own its magnetiſm to ſome ſmall quantity of iron concealed in its ſubſtance.

Nickel is a metallic ſubſtance which has been ſuſpected to be capable of acquiring ſome degree of magnetiſm independent of iron; and this ſuſpicion has been founded upon obſerving, that nickel retained its magnetiſm after having been repeatedly purified*. There are, however, perſons who have denied the magnetiſm of purified nickel; and I have ſeen ſome pieces of it which did not in the leaſt affect the magnetic needle. It is probable, that thoſe pieces were not pure nickel, and perhaps ſome cobalt was contained in them; but I ſee no reaſon why the nickel, when alloyed with a little cobalt, ſhould ſhew no magnetiſm, if that property does really belong eſſentially to it.

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* See KIRWAN's Mineralogy, p. 342. and 367.

The greateſt number of my experiments are relative to the properties of braſs; and they ſeem to prove, that this compound metal, which is often magnetic, does not owe its magnetiſm to iron, but to ſome particular configuration of its component particles, occaſioned by the uſual method of hardening it, which is by hammering.

In ſome ſpecimens of braſs, and eſpecially in that which has often paſſed from the work-ſhop to the furnace, and from the latter to the former, there are ſometimes pieces of iron ſenſible not only to the magnet, or the chemical analyſis, but even to the ſight, which render the braſs ſtrongly magnetic. But the braſs generally uſed in my experiments was ſuch as, when quite ſoft, it had no ſenſible degree of magnetiſm.

Before we begin with the narration of thoſe experiments, it will be proper to deſcribe the magnetic needle I generally uſed; which is ſuſpended in a particular manner; and which may be uſeful to perſons who are fond of making magnetic experiments, not only for its ſenſibility, but likewiſe for the ſimplicity of its conſtruction.

Experience having ſhewn, that large magnetic needles are not proper for experiments wherein a very ſmall degree of magnetiſm muſt be aſcertained, and the free motion of the uſual ſmall needles being proportionally more obſtructed by the nature of their ſuſpenſion, even when furniſhed with agate caps, I endeavoured to contrive a ſort of ſuſpenſion which might anſwer the purpoſe better than the needles ſuſpended in the uſual manner; and, after ſeveral attempts, at laſt I conſtructed a chain of horſe-hair, conſiſting of five or ſix links, to which the needle was ſuſpended. Each link is about three-quarters of an inch in diameter; and the extremities of each piece of hair, which is formed in a ring, are joined by a knot, and ſecured by a little ſealing-wax. The link on one end of this chain is ſuſpended to a pin in a proper frame, or any ſupport that may be at hand; and to the link of the other extremity which lies lowermoſt, a piece of fine ſilver wire is hooked. This wire is about an inch and a half long, and its lower extremity is faſtened round a ſmall and cylindrical piece of cork, through which a common ſewing needle, made magnetic, is thruſt horizontally. Thus the magnetic needle is ſuſpended by a hair-chain, the links of which, on account of the ſmoothneſs and lightneſs of the hair, move very freely in each other, and allow the needle more than a whole revolution round its centre, with ſo ſmall a degree of friction as may be conſidered next to nothing. By comparing this needle with others of the beſt ſort in uſe, I find the former to be much more ſenſible; for when bodies which have an exceedingly ſmall magnetic power are tried, this needle will be frequently attracted by them when the others are not ſenſibly affected.

In order to try farther the delicacy of ſuch ſuſpenſion, I placed a piece of looking-glaſs under the needle, and nearly horizontal, ſo that the image of the needle was ſeen in it. Now, as a fine line had been previouſly marked on the glaſs, things were ſo diſpoſed as that the image of the needle might coincide with the line marked on the glaſs, the eye being placed in a proper point of view; afterwards, by ſhaking the needle either very gently or very quickly, I repeatedly endeavoured to place it out of the magnetic meridian; but every endeavour proved ineffectual, for the needle conſtantly ſettled in the ſame direction, without any ſenſible variation.

With a needle thus ſuſpended a variation compaſs might be very eaſily conſtructed, and it would perhaps be more accurate than thoſe commonly in uſe. For this purpoſe the needle ought to be about three inches long, and the piece of looking-glaſs ought to be fixed upon the index of an HADLEY's ſextant, which muſt be placed horizontally under the needle, with its edge or fiducial line in the meridian of the place, in order to obſerve the daily variation of the needle. I have made only a rough model of ſuch a variation compaſs, and it ſeemed to anſwer very well. This conſtruction appears to have the following advantages over the common ſort: 1ſt, the needle being cylindrical, and without a hole in the middle, would be leſs ſubject to have more than two poles. 2dly, The needle being ſlender, its poles would ſtand more exactly in its axis, which with the common flat needles is ſeldom the caſe. 3dly, It will appear, by a little conſideration, that in this conſtruction there is no need of the needle's center of motion keeping always in the ſame invariable point, which renders the conſtruction both very eaſy and very accurate: and, laſtly, as the ſextant may be placed at a conſiderable diſtance below the needle, and the reſt of the frame may be made of any ſize, there would be no neceſſity of placing any braſs or other metal ſo near the needle as might affect it in caſe this metal had any magnetiſm, which generally happens with braſs.

In order to examine the magnetiſm of divers ſubſtances, beſides the above deſcribed needle, I uſed to put a ſmall magnetic needle upon water, and then bring the ſubſtance to be examined near it, or place the ſubſtance itſelf upon water,

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ſometimes reſting it upon pieces of cork, and then bring a powerful magnet near it.

Examination of the Magnetical Properties of Braſs.

A few years ago, being intent on making ſome magnetic experiments, in which braſs was concerned, I uſed to examine firſt whether the pieces of braſs had any magnetiſm or not, and rejected thoſe pieces which had an evident degree of that power. In the courſe of thoſe experiments I remember to have obſerved, that thoſe pieces of braſs which had been hammered were generally magnetic, and much more ſo than others; in conſequence of which I made no uſe of hammered braſs in thoſe experiments. But lately, having ordered a theodolite at a philoſophical inſtrument ſhop, I particularly enjoined the workmen to try the braſs, both ſoft and hammered, before they worked it, and to make no uſe of that which had any magnetiſm. They found, that hammered braſs, even ſuch as before the hammering had no magnetiſm, could afterwards diſturb the magnetic needle very ſenſibly. Theſe obſervations induced me to make the following experiments.

EXPERIMENT I.

An oblong piece of braſs, weighing ſomewhat leſs than half an ounce, being examined by preſenting every part of its ſurface to the ſuſpended needle, ſhewed no ſign of magnetiſm whatever. It was then hammered for about two minutes; the conſequence of which was, that it became magnetic ſo far as to attract either end of the needle from about a quarter of an inch, This ſame piece of braſs being now put into the fire ſo as to become red-hot, by which means it was ſoftened, and when cold being preſented to the ſuſpended needle, its magnetiſm was found to be entirely gone. Hammering made it again magnetic. Softening by fire took the magnetiſm away a ſecond time; and thus the magnetiſm was repeatedly given it by hammering, and was deſtroyed by ſoftening; ſometimes ſhewing to have acquired a ſenſible degree of that power, even after two or three ſtrokes of the hammer.

EXPERIMENT II.

The reſult of the firſt experiment would naturally induce one to ſuſpect, that the hammer and anvil might have imparted ſome ſmall quantity of ſteel to the braſs, which rendered it magnetic; and that this magnetiſm was deſtroyed in ſoftening the braſs, inſomuch as the fire calcined the ſmall quantity of ſteel that had adhered to it. In conſequence of which conſideration, I took other pieces of braſs beſides that uſed before, and hammered them between card-paper, changing the pieces of paper as often as was neceſſary, ſince they were eaſily broken by the hammer; but the pieces of braſs became conſtantly magnetic by the hammering, and their magnetiſm was deſtroyed by fire.

In this experiment I generally gave to the braſs not above thirty ſtrokes with the hammer.

EXPERIMENT III.

Still ſuſpecting that the hammer and the anvil might have imparted ſome ſmall quantity of iron to the braſs, becauſe the pieces of card-paper ſometimes were broken by the firſt or

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ſecond ſtroke, in which caſe either the hammer or the anvil touched the braſs; I hardened a piece of braſs by beating it between two large flints, *viz.* uſing one for the hammer, and the other for the anvil. The piece of braſs became magnetic, though in this caſe it ſeemed to have acquired not ſo much power as when it had been hardened with the hammer; but it muſt be obſerved, that the flints being rough and irregular, the piece of braſs could not be hardened by them ſo eaſily, or ſo equally, as by the other method.

The flints, being examined both before and after the experiment, were found to have not the leaſt degree of magnetiſm.

EXPERIMENT IV.

A piece of braſs, which by hammering had been rendered ſo ſtrongly magnetic as to attract either pole of the needle from about a quarter of an inch, was put into a crucible, together with a conſiderable quantity of charcoal duſt, which ſurrounded it every where. The crucible was covered with clay, and being put into the fire, was kept red-hot for about ten minutes. After cooling, the piece of braſs was taken out of the crucible, and being examined, was found to have entirely loſt its magnetiſm. The object of this experiment was to aſcertain whether the loſs of magnetiſm, in a piece of braſs that was ſoftened, was owing to the calcination of the ferrugineous particles, which, notwithſtanding the preceding experiments, might ſtill be ſuſpected to be imparted to it; becauſe in this way of ſoftening the braſs, the ferrugineous particles being ſurrounded with charcoal duſt, could not have been calcined; hence the braſs ought not to have loſt its magnetiſm, which was not the reſult of the experiment.

EXPERIMENT V.

One of thoſe pieces of braſs which had been uſed for the foregoing experiments, and which had been deprived of magnetiſm by fire, was hammered between two large and pretty thick pieces of copper, which were not in the leaſt magnetic; and, after a few ſtrokes of the hammer, it became ſenſibly magnetic.

EXPERIMENT VI.

In order to examine the difference of this property in braſs of various kinds, I have tried a great many pieces of Engliſh as well as foreign braſs; ſome of which was very old, and ſo fine and uniform, that an eminent watch-maker of my acquaintance uſed it for the very beſt ſort of watch work. But I find, that they moſtly have the property of becoming magnetic by hammering, and of loſing that power when ſoftened. There are, however, ſome pieces which acquire no magnetiſm by the hammering, though they are rendered equally hard by it as thoſe which acquire the magnetiſm. By attentively examining them, I have not yet been able to diſtinguiſh, without a trial, which pieces are capable of acquiring magnetiſm, and which not; the colour, apparent texture, and degree of ductility ſeeming to afford no ſure indication. In ſhort, what I have obſerved relating to the magnetic properties of braſs is:

1ſt, That moſt braſs becomes magnetic by hammering, and loſes the magnetiſm by annealing or ſoftening in the fire.

2dly, That the acquired magnetiſm is not owing to particles of iron or ſteel imparted to the braſs by the tools employed.

3dly, Thoſe pieces of braſs which have that property, retain it without any diminution after a great number of repeated trials, *viz.* after having been repeatedly hardened and ſoftened. But I have not found any means to give that property to ſuch braſs as had it not naturally.

4thly, A large piece of braſs has generally a magnetic power ſomewhat ſtronger than a ſmaller piece; and the flat ſurface of the piece draws the neeedle more forcibly than the edge or corner of it.

5thly, If only one end of a large piece of braſs be hammered, then that end alone will diſturb the magnetic needle, and not the reſt.

6thly, The magnetic power which braſs acquires by hammering has a certain limit, beyond which it cannot be increaſed by farther hammering. This limit is various in pieces of braſs of different thickneſs, and likewiſe of different quality.

7thly, Though there are ſome pieces of braſs which have not the property, of being rendered magnetic by hammering; yet all the pieces of magnetic braſs, that I have tried, loſe their magnetiſm by being made red-hot, excepting indeed when ſome piece of iron is concealed in them, which ſometimes occurs; but in this caſe, the piece of braſs, after having been made red-hot and cooled, will attract the needle more forcibly with one part of its ſurface than with the reſt of it; and hence, by turning the piece of braſs about, and preſenting every part of it ſucceſſively to the ſuſpended magnetic needle, one may eaſily diſcover in what part of it the iron is lodged.

From thoſe obſervations it follows, that when braſs is to be uſed for the conſtruction of inſtruments wherein a magnetic needle is concerned, as dipping needles, variation compaſſes, &c. the braſs ſhould be either left quite ſoft, or it ſhould be choſen of ſuch a ſort as will not be made magnetic by hammering, which ſort however does not occur very eaſily.

Examination of the Magnetic Properties of ſome other Metallic Subſtances.

The reſult of the experiments on braſs induced me to examine other metallic ſubſtances, and eſpecially its components, *viz.* copper and zinc: though the reſult of the experiments has not been very remarkable, excepting with platina, which metal has properties in great meaſure analogous to thoſe of braſs.

Having examined various pieces of copper, by means of the ſuſpended magnetic needle, and having never found them magnetical, except only ſometimes in ſuch places which had been filed, and where ſome particles of ſteel might have been left by the file, I next proceeded to hammer ſome pieces of it, not only in the uſual way, but likewiſe between flints: the reſult, however, was very dubious; for though, in general, they had no effect whatever on the needle, yet ſometimes I thought the needle was really attracted by ſome pieces of hammered copper; but then this attractive power was ſo exceedingly ſmall as not to be depended upon.

Zinc, either not hammered, or hammered as far as could be done without breaking it, ſhewed no ſigns of magnetiſm whatever, when preſented to the magnetic needle. A mixture of zinc and tin neither had any action upon the needle.

A piece of a broken reflector of a teleſcope, which conſiſted of tin and copper; a mixture of tin, zinc, and a little copper; a piece of ſilver, both ſoft and hammered; a piece of pure gold, both ſoft and hammered; a mixture of gold and ſilver, both hard and ſoft; and another mixture of a great deal of

ſilver,

ſilver, a little copper, and a leſs quantity of gold, either before or after hammering, had not the leaſt action on the magnetic needle.

Platina was the metal I laſt examined, and the experiments made with it ſeem to deſerve particular attention.

EXPERIMENT I.

A large piece of platina, which, after being precipitated from its ſolution in *aqua regia*, had been fuſed, or rather concreted together, being preſented to the ſuſpended magnetic needle, ſhewed not the leaſt ſign of magnetiſm. It was then hammered; but after the third or fourth ſtroke of the hammer it broke into many pieces, ſeveral of which being tried, ſhewed no magnetiſm, nor could any of the fineſt particles be attracted by the magnet preſented very nearly over them. The broken ſurface of this piece of platina was full of cavities, ſome of which were large, and others juſt diſcernible; and altogether the metal ſeemed to have undergone an imperfect fuſion.

EXPERIMENT II.

The grains of native platina were examined next, by putting a magnet juſt over them; but the magnet attracted not above ten or twenty particles out of about half an ounce of platina: and thoſe which were attracted had either little or no ſhining metallic appearance like the reſt, and were exceedingly ſmall.

EXPERIMENT III.

Having picked out ſeveral of the largeſt grains of platina, I preſented the magnet to them; but they were not

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in the leaſt attracted by it. One of thoſe grains was then hammered; by which means, after about eight or ten ſtrokes, it was ſpread into a plate, about a tenth of an inch in diameter, and nearly circular; afterwards the magnet being preſented to it, the former attracted it from the diſtance of about one-twentieth of an inch. The other grains being all hammered one after the other, were rendered by it ſo far magnetic as to be attracted by the magnet, and to diſturb the ſuſpended needle when they were preſented to it. But there were ſome amongſt them which acquired no magnetiſm at all, though they had been purpoſely hammered much longer than the others.

As far as I could obſerve, thoſe pieces which would not acquire any magnetiſm by hammering, had not a very ſhining appearance before the hammering, though afterwards they could not be diſtinguiſhed from the others by their appearance; and they ſeemed not to ſpread under the hammer ſo eaſily as the others.

In general three or four ſtrokes are ſufficient to render a grain of platina evidently magnetic, but about ten ſtrokes give it the full power it is ſuſceptible of.

EXPERIMENT IV.

Thoſe grains of platina, which in the preceding experiments had been rendered magnetic by hammering, being put upon a charcoal, were made red-hot by means of a blow-pipe; and afterwards being preſented to the magnet, and likewiſe to the ſuſpended needle, they ſhewed not the leaſt ſign of magnetiſm. Heat, therefore, deprives them as well as braſs of the magnetiſm acquired by hammering. A ſecond hammering rendered them magnetic, though not ſo quickly, nor to ſo great a degree, as it had done the firſt time. However, it muſt be obſerved, that the pieces of platina having been rendered flat and thin by the firſt hammering, could not be ſo eaſily ſtruck, nor ſpread much more, by the ſecond.

If it is true, as thoſe experiments ſeem to prove beyond a doubt, that magnetiſm may exiſt, or may belong to other ſubſtances, independent of iron, it muſt follow, that the attraction of a few particles of an unknown ſubſtance by the magnet is not a ſure ſign of the preſence of iron. Hence thoſe ſubſtances, which hitherto have been conſidered as containing ferrugineous particles, for no other reaſon but becauſe the magnet attracted a ſmall quantity of them, muſt be conſidered as dubious; and the concluſion of the exiſtence of iron ſhould not be admitted, except when thoſe particles, which have been ſeparated by the magnet, appear to be iron by ſome other trial; for though it is true, that iron is always attracted by the magnet, yet it does not hence follow, that whatever is attracted by the magnet muſt be iron.

POSTSCRIPT.

THE exiſtence of magnetiſm, or of the power of attracting and being attracted by the magnetic needle, in bodies, without the interference of iron or any ferrugineous matter, being a propoſition not only new and ſingular, but ſeemingly of importance in philoſophy, the experiments which tend to confirm it ſhould be never deemed ſuperfluous, nor any poſſible objection be left unanſwered: hence, ſince the writing of the foregoing paper, I have endeavoured to raiſe objections, and to contrive means of explaining them; but every conſideration ſeemed to confirm the propoſition advanced. The principal of thoſe objections was, that the braſs which becomes magnetic by hammering and loſes that power by ſoftening, might contain a ſmall quantity of iron, to which that magnetic power was owing; and that this iron or martial earth, diſperſed through the ſubſtance of the braſs, might become phlogiſticated by the action of hammering; inſomuch as the braſs being forced into a ſmaller ſpace might perhaps give ſome of its phlogiſton to the martial earth, and thus render it magnetic; and, on the contrary, the action of the fire in ſoftening might remove that phlogiſton from the martial earth, and give it to the braſs; hence the former, remaining quite dephlogiſticated, would no longer ſhew any ſigns of magnetiſm. The conſideration that iron may be dephlogiſticated or calcined more eaſily than braſs gave an apparent weight to the ſuppoſition; but the following experiments ſeem to expel every doubt.

EXPERIMENT I.

Having choſen a piece of braſs which would acquire no magnetiſm by hammering, I placed it upon the anvil, together with a conſiderable quantity of *crocus martis*, which crocus had no action on the magnetic needle; then began hammering the braſs, and turning it frequently, in order to let part of the crocus adhere to it; and, in fact, the crocus had in ſeveral places been faſtened ſo well into the braſs, that hard wiping with a woollen cloth would not rub it off. The braſs appeared red in thoſe places; but, after having been hammered for a long time, it acquired no magnetiſm whatever. The hardening, therefore,

fore, could not render the iron calx ſo far phlogiſticated as to affect the magnetic needle.

EXPERIMENT II.

In order to diverſify the preceding experiment, I drilled a hole, about one-eighth of an inch long, and little more than one-fiftieth of an inch in diameter, into a piece of braſs that was not rendered magnetic by hammering; and filled it with *crocus martis*; then I hammered the piece of braſs, thus incloſing the calx of iron, and afterwards preſented it to the needle; but there was not the leaſt ſign of attraction: the martial earth, therefore, had not acquired any phlogiſton from the braſs by the action of hammering.

EXPERIMENT III.

The piece of braſs mentioned in the preceding experiment, *viz.* with a little calx of iron in it, was put into the fire, and was made quite red-hot, in which ſtate it remained for about three minutes. Then, after cooling, it was preſented to the magnetic needle, and this was attracted by the braſs only in that place wherein the calx of iron was contained. The action, therefore, of the fire had rendered the martial earth ſo far phlogiſticated as to attract the magnetic needle; hence, if the magnetiſm of braſs was owing to any ferrugineous matter contained in it, a piece of braſs ought to become magnetic when ſoftened, which is contrary to the experiments mentioned in this paper.

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EXPERIMENT IV.

A hole, ſimilar to that mentioned in the ſecond experiment, was drilled into a piece of braſs that would not become magnetic by hammering, and into it was put ſome black calx of iron, which was ſo far phlogiſticated as to be attractable by the magnet, and the hole was cloſed by a few ſtrokes of the hammer. In conſequence of which the piece of braſs, when preſented to the ſuſpended magnetic needle, would attract it only about that place where the magnetic calx was contained. This attraction was very weak. Then the piece of braſs, thus prepared, was put into the fire, and was kept for about ſix minutes, in a heat very little ſhort of that neceſſary to melt braſs, and after cooling I preſented it to the needle, expecting that the fire might have dephlogiſticated that calx of iron ſo far as not to let it act any longer upon the needle; but the attraction appeared to be of the ſame degree it was before the heating.

It ſeems, therefore, to be demonſtrated, as far as the ſubject will admit of demonſtration, that the magnetiſm acquired by braſs, when hammered, is not owing to iron contained in it; and, conſequently, that *magnetiſm, or the power of being attracted by, and attracting, the magnet, may exiſt independent of iron.*

TO DR. BLAGDEN, SEC. R. S.

SIR, Windſor, January 9, 1786.

I HAVE made the experiment which you recommended me to try, relating to the magnetiſm of braſs; *viz.* I mixed, by means of the blow-pipe, a ſmall quantity of iron, with about four times its weight of ſuch braſs as would not become magnetic by hammering. The whole globule weighed about two grains, and it attracted the magnetic needle very powerfully. I then melted this globule of braſs and iron with about fifty grains of the ſame ſort of braſs as had been uſed before. After cooling, the whole lump of braſs appeared to have very little power upon the magnetic needle, every part of its ſurface attracting one end of the ſuſpended needle, ſo as to let it juſt adhere to it when the air was not at all diſturbed. But this weak and hardly perceivable degree of magnetiſm was not increaſed by hammering, nor annihilated by ſoftening.

In the courſe of my experiments on the magnetiſm of braſs, I have twice obſerved the following remarkable circumſtance. A piece of braſs, which had the property of becoming magnetic by hammering, and of loſing the magnetiſm by ſoftening, having been left in the fire till it was partially melted, I found, upon trial, that it had loſt the property of becoming magnetic by hammering; but having been afterwards fairly melted in a crucible, it thereby acquired the property it had originally, *viz.* that of becoming magnetic by hammering.

I have likewiſe often obſerved, that a long continuance in a fire ſo ſtrong as to be little ſhort of melting hot, generally diminiſhes, and ſometimes quite deſtroys, the property of becoming magnetic in braſs. At the ſame time, the texture of the metal is conſiderably altered, becoming what ſome workmen call *rotten*. From this it appears, that the property of becoming magnetic in braſs by hammering, is rather owing to ſome particular configuration of its parts, than to the admixture of any iron; which is confirmed ſtill farther by obſerving, that Dutch plate-braſs (which is made not by melting the copper, but by keeping it in a ſtrong degree of heat whilſt ſurrounded by *lapis calaminaris*) alſo poſſeſſes that property; at leaſt all the pieces of it, which I have tried, have that property.

I am, &c.

T. CAVALLO.

IV. *On Infinite Series.* *By* Edward Waring, *M. D. F. R. S.* Lucaſian *Profeſſor of Mathematics at* Cambridge.

Read December 15, 1785.

1. IN the Paper, which the Royal Society did me the honour to print, on Summation of Series, is given a method of finding the ſum of a ſeries, whoſe general term $\left(\frac{P}{Q}\right)$ (where $\frac{P}{Q}$ is a fraction reduced to its loweſt terms) is a determinate algebraical function of the quantity (z) the diſtance from the firſt term of the ſeries, which always terminates when the ſum of the ſeries can be expreſſed in finite terms.

2. The terms of every infinite ſeries muſt neceſſarily be given by a function of z, or by quantities which can be reduced to a function of z.

3. Let $Q = A \times A' \times A'^{2} \times \,..\, A'^{n} \times B \times B' \times B'^{2} \times \,...\, B'^{m} \times C$ $C' \times C'^{2} \times \,..\, C'^{r} \times$ &c. where A', A'^{2}, $A'^{3} \,...\, A'^{n}$, are ſucceſſive values of A; that is, reſult from A by writing in it for z reſpectively $z+1$, $z+2$, $z+3$, $..\, z+n$; and B', B'^{2}, B'^{3}, B'^{m}, reſult from B, by writing in it for z reſpectively $z+1$, $z+2$, $z+3$, $...\, z+m$; but B is not a ſucceſſive value of A; &c. Let the numerator $P = E \,.\, E' \,.\, E'^{2} \,..\, E'^{b-1} \,.\, F \,.\, F' \,.\, F'^{2} \,...\, F'^{k-1} \times L$; E', $E'^{2} \,..\, E'^{b-1}$; F', $F'^{2} \,..\, F'^{k-1}$, &c. denoting ſucceſſive values of the quantities E, F, &c. reſpectively; and L, admitting of no diviſor of the formula $K \times K'$, where K' is a ſuc-

ceſſive value of K: let $L = A \times B \times C \times \&c. \times E^{1b} \times F^{1k} \times \&c. \times p' \times q' \times r' \times \&c. - A^{1n} \times B^{1m} \times C^{1r} \times \&c. \times E \times F \times \&c. \times p \times q \times r \times \&c.$ where p', q', r', &c. are irrational quantities and ſucceſſive values of p, q, r, &c. The factors A B, C, &c. E^{1b}, F^{1k}, &c. being given, the factors p', q', r', &c. into which they are multiplied in the quantity L will eaſily be deduced by deducting the preceding irrational factors contained in A, B, C, &c. E^{1b}, F^{1k}, &c. from the correſpondent irrational factors contained in L; and in the ſame manner, from the factors A^{1n}, B^{1m}, &c. E, F, &c. can be deduced the irrational factors of the preceding p, q, r, &c.

Aſſume for the ſum of the ſeries ſought the quantity

$$\frac{E \times E' \times E^{12} \,..\, E^{1b-1} \times F \times F' \,..\, F^{1k-1} \times \&c.}{A \,.\, A' \,.\, A^{12} \,..\, A^{1n-1} \times B \times B' \times B^{1.2} \,..\, B^{1m-1} \times C \times C' \,..\, C^{1.r-1} \times \&c. \times p \times q \times r \times s \times \&c.} (\alpha z^{m'} + \beta z^{m'-1} + \gamma z^{m'-2} + \&c.) = V;$$

where m' is a whole number, and α, β, γ, &c. are co-efficients to be inveſtigated; write in V for z its ſucceſſive value $z+1$, and let the reſult be W; reduce the difference $W - V$ into a fraction in its loweſt terms, and make the co-efficients of the correſpondent terms of the reſulting fraction $= (W - V)$ and of the given fraction equal to each other, if poſſible; and thence may be deduced the ſum of the ſeries required.

4. This ſeries will terminate if the ſum ſought can be expreſſed by a finite determinate function of z; if not, it will proceed *in infinitum*, and may be expreſſed either by a ſeries aſcending or deſcending according to the dimenſions of z.

5. If any factor, A or B, or C, &c. have no ſucceſſive one in the denominator; or if the greateſt dimenſions of z in the denominator be greater than its greateſt dimenſions in the numerator by 1, then the ſum of the ſeries is not a finite algebraical function of z.

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6. If in the denominator are deficient ſome intermediate ſucceſſive factors, multiply both the numerator and denominator by thoſe deficient factors, and they are ſupplied: for example, let $A\times A'''\times A''''''\times$ &c. be factors of the denominator, in which are deficient the factors A', A'', A'''', &c. multiply both numerator and denominator by the content $A'\times A''\times A''''\times$ &c. and they are reſtored.

7. If in the denominator is contained the content $A^{l}\times A'^{l'}\times A''^{l''}\ldots A'^{n\,l^{n}}=H$, in which λ is the leaſt of the indices l, l', l'', &c.: aſſume $A^{\lambda}\times A'^{\lambda}\times A''^{\lambda}\ldots A'^{n\,\lambda^{n}}\times\alpha=H$, and in the ſame manner reduce the factors in α; then aſſume for the denominator $A^{\lambda}\times A'^{\lambda}\times A''^{\lambda}\ldots A'^{n-1\,\lambda}\times$ &c.: for example, let the contents be $A^{2}\times A'^{5}\times A''^{2}\times{}'''^{5}\times$ &c. $=H$, then is 2 the leaſt index, and conſequently the content reduced as before taught will be $A^{2}\,.\,A'^{2}\,.\,A''^{2}\,.\,A'''^{2}\times\alpha=A^{2}\,.\,A'^{2}\,.\,A''^{2}\,.\,A'''^{2}\times A\,.\,A'\,.\,A'''\times A'^{2}\,.\,A'''^{2}$; but between the factors A' and A''' is deficient the factor A''; and between the factors A'^{2} and A'''^{2} is deficient the factor A''^{2}; multiply the numerator and denominator of the given fraction by the deficient factors $A''\times A''^{2}$; aſſume for the denominator $A^{2}\,.\,A'^{2}\,.\,A''^{2}\times A\,.\,A'\,.\,A''\times A'^{2}\,.\,A''^{2}\times$ &c. $=A^{3}\,.\,A'^{5}\,.\,A''^{5}\times$ &c. &c.

8. If the greateſt index of the content H is contained in one factor only, then the ſum of the ſeries cannot be expreſſed in finite terms of the quantity z.

9. The ſame may be applied to the contents of the ſeveral ſucceſſive values of the quantities B, C, &c. in the denominator: for example, let the general term be $\frac{1}{1\,.\,2\,.\,3\,.\,.\,\overline{z-2}\times z}$; multiply it into $z-1$ to compleat the deficient term, and it reſults

$\frac{z-1}{1\cdot 2\cdot 3\,..\,z}$; aſſume, by the preceding method for the ſum of the ſeries the quantity $\frac{1}{1\cdot 2\cdot 3\,..\,z-1}$, of which the ſucceſſive term is $\frac{1}{1\cdot 2\cdot 3\,..\,z}$, and their difference $\frac{1}{1\cdot 2\,..\,z-1}-\frac{1}{1\cdot 2\,..\,z}$ $=\frac{1}{1\cdot 2\,..\,z-2\times z}$ the given term.

2. Let the term be Ne^{z} and e leſs than 1, which is the term of a geometrical ſeries; then will the ſum of the infinite ſeries be $\frac{N}{1-e}\times e^{z}$, beginning from the term whoſe diſtance from the firſt is z; for the difference between the two ſucceſſive ſums $=$ $\frac{N}{1-e}(e^{z}-e^{z+1})=Ne^{z}$ the given term.

3. Let the general term be $\frac{(z+n+1-e^{n}\times\overline{z+1})a}{z+1\cdot z+n+1}\times e^{z+1}$; aſſume for the ſum of the ſeries the ſubſequent quantity $(z+1\cdot z+2\cdot z+3\,...\,z+n)^{-1}\times e^{z}\times(\alpha+\beta z+\gamma z^{2}\,..\,z^{n-1})$ and by the preceding method the co-efficients α, β, γ, &c. may be found: the ſum is known to be $=\frac{a}{z+1}\times e^{z+1}+\frac{a}{z+2}\times e^{z+2}+\frac{a}{z+3}\times e^{z+3}$ $..\,\frac{a}{z+n}e^{z+n}$, which can eaſily be reduced to the preceding formula.

If the general term be $\frac{T'-T}{T\times T'}$ or $\frac{PQ'-QP'}{QQ'}$; where T and T′, P and P′, Q and Q′, are ſucceſſive terms; then will the ſums of the ſerieſes be $\frac{1}{T}$ or $\frac{P}{Q}$ properly corrected.

10. If the function expreſſing the general term contain in the denominator a factor or factors, which have no ſucceſſive one; reduce the factor or factors into an infinite ſeries proceeding according to the dimenſions of z, and thence, by the method before given, find the ſum of the ſeries. The ſame method

may

may be purſued, when the denominator of a fluxion, which is a function of x multiplied into $\dot{x}$ contains the ſimple power only of a factor or factors; reduce the factor or factors into an infinite ſeries, proceeding according to the dimenſions of x, and by the known methods find the fluent of the fluxion.

11. The fluent of the fluxion or ſum of the ſeries may be deduced alſo from the ſubſequent propoſitions, from which may be inveſtigated many ſerieſes, whoſe ſums are known.

1. Let $p\dot{P} = Q\dot{q}$, $q\dot{Q} = R\dot{r}$, $r\dot{R} = S\dot{s}$, $s\dot{S} = T\dot{t}$, &c.; then $\int P\dot{p} = Pp - Qq + Rr - Ss + Tt - \&c.$ if only the ſeries converges.

2. Let $pP' + p'P' = Qq'$, $qQ' + q'Q' + = Rr'$, $rR' + r'R' = Ss'$, $sS' + s'S' = Tt'$, &c. where P', p', Q', q', &c. denote the increments of the quantities P, p, Q, q, &c. reſpectively, then will the integral of the increment $(Pp') = Pp - Qq + Rr - Ss +$ &c. if only the ſeries converges.

Ex. 1. $\int x^{-n}\dot{x} = \int\frac{x^m\dot{x}}{x^{n+m}} = \frac{1}{x^{n-1}}\left(\frac{1}{m+1} + \frac{n+m}{r'+1}A + \frac{n+r'}{s'+1}B + \frac{n+s'}{t'+1}C + \frac{n+t'}{T+1}D + \&c.\right) = \frac{1}{1-nx^{n-1}}$; whence $\frac{1}{1-n} = \frac{1}{m+1} + \frac{n+m}{r'+1}\times\frac{1}{m+1}$ $\frac{n+r'}{s'+1}\times\frac{n+m}{r'+1}\times\frac{1}{m+1} + \&c.$ In this example $\dot{p} = x^m\dot{x}$, $P = x^{-n-m}$, $\dot{q} = x^{r'}\dot{x}$, $Q = x^{-n-r'}$, $\dot{r} = x^{s'}\dot{x}$, &c.

Ex. 2. $\int\frac{x^m\dot{x}}{x^{n+r}} = \int x^{m-n-r}\dot{x} = \frac{1}{m-n-r+1}x^{m-n-r+1} = x^{m-n-r+1}\left(\frac{1}{m+1} + \frac{1}{m+1}\times\frac{n+r}{m+2} + \frac{1}{m+1}\cdot\frac{n+r}{m+2}\cdot\frac{n+r+1}{m+3} + \&c.\right)$; whence $\frac{1}{m-n-r+1} = \frac{1}{m+1} + \frac{n+r}{m+2}A + \frac{n+r+1}{m+3}B + \&c.$ In both theſe examples the letters A, B, C, &c, denote the preceding terms.

Ex. 3. $\int\frac{x^m\dot{x}}{1+x} = \frac{x^{m+1}}{1+x}\left(\frac{1}{m+1} + \frac{1}{m+1\cdot m+2}\times\frac{x}{1+x} + \frac{2}{m+1\cdot m+2\cdot m+3}\times\frac{x^2}{(1+x)^2} + \frac{2\cdot 3}{m+1\cdot m+2\cdot m+3\cdot m+4}\cdot\frac{x^3}{(1+x)^3} + \&c.\right)$

Ex. 4. $\int \frac{a'x^{m}\dot{x}}{(a+bx^{n}+cx^{2n})^{b}} = \frac{1}{m+1}\,\frac{a'x^{m+1}}{(a+bx^{n}+cx^{2n})^{b}} + \frac{a'x^{m+1}}{m+1\,.\,(a+bx^{n}+cx^{2n})^{b+1}} \times \left(\frac{nbbx^{n-1}}{m+n+1} + \frac{2nbcx^{2n-1}}{m+2n+1}\right) + \&c.$

12. Let the general term of an infinite ſeries be

$$\frac{A}{z+\alpha\,.\,z+\alpha+1\,.\,z+\alpha+2\,..\,z+\alpha+n \times z+\beta\,.\,z+\beta+1\,.\,z+\beta+2\,..\,z+\beta+m \times z+\gamma\,.\,z+\gamma+1\,.\,z+\gamma+2\,..\,z+\gamma+r \times z+\delta\,.\,z+\delta+1\,.\,z+\delta+2\,..\,z+\delta+s \times \&c.};$$

where $\alpha-\beta$, $\alpha-\gamma$, $\beta-\gamma$, &c. are not whole numbers; the ſum of the ſeries can always be expreſſed in finite terms; if the ſum of the fractions

$$\frac{1}{1\,.\,2\,.\,3\,..\,n} \times \frac{1}{\beta-\alpha\,.\,\beta-\alpha+1\,.\,\beta-\alpha+2\,..\,\beta-\alpha+m \times \gamma-\alpha\,.\,\gamma-\alpha+1\,.\,\gamma-\alpha+2\,..\,\gamma-\alpha+r \times \delta-\alpha\,.\,\delta-\alpha+1\,.\,\delta-\alpha+2\,..\,\delta-\alpha+s \times \&c.}$$

$$- \frac{1}{1 \times 1\,.\,2\,.\,3\,..\,n-1} \times \frac{1}{\beta-\alpha-1\,.\,\beta-\alpha\,.\,\beta-\alpha+1\,.\,\beta-\alpha+2\,..\,\beta-\alpha+m-1 \times \gamma-\alpha-1\,.\,\gamma-\alpha\,.\,\gamma-\alpha+1\,..\,\gamma-\alpha+r-1 \times \delta-\alpha-1\,.\,\delta-\alpha\,.\,\delta-\alpha+1\,..\,\delta-\alpha+s-1 \times \&c.} +$$

$$\frac{1}{1\,.\,2 \times 1\,.\,2\,.\,3\,..\,n-2} \times \frac{1}{\beta-\alpha-2\,.\,\beta-\alpha-1\,.\,\beta-\alpha\,.\,\beta-\alpha+1\,..\,\beta-\alpha+n-2 \times \gamma-\alpha-2\,.\,\gamma-\alpha-1\,.\,\gamma-\alpha\,...\,\gamma-\alpha+n-2 \times \delta-\alpha-2\,.\,\delta-\alpha-1\,.\,\delta-\alpha\,..\,\delta-\alpha+n-2 \times \&c.}$$

$$- \frac{1}{1\,.\,2\,.\,3 \times 1\,.\,2\,..\,n-3} \times \frac{1}{\beta-\alpha-3\,.\,\beta-\alpha-2\,..\,\beta-\alpha+n-3 \times \gamma-\alpha-3\,.\,\gamma-\alpha-2\,..\,\gamma-\alpha+n-3 \times \delta-\alpha-3\,.\,\delta-\alpha-2\,..\,\delta-\alpha+s-3 \times \&c.} + \ldots \frac{1}{1\,.\,2\,.\,3\,..\,n} \times$$

$$\frac{1}{\beta-\alpha-n\,.\,\beta-\alpha-n+1\,..\,\beta-\alpha-n+m \times \gamma-\alpha-n\,.\,\gamma-\alpha-n+1\,..\,\gamma-\alpha-n+r \times \delta-\alpha-n\,.\,\delta-\alpha-n+1\,..\,\delta-\alpha-n+s \times \&c.} = 0;$$

$$\frac{1}{1\,.\,2\,.\,3\,..\,m} \times \frac{1}{\alpha-\beta\,.\,\alpha-\beta+1\,.\,\alpha-\beta+2\,..\,\alpha-\beta+m \times \gamma-\beta\,.\,\gamma-\beta+1\,..\,\gamma-\beta+r \times \delta-\beta\,.\,\delta-\beta+1\,.\,\delta-\beta+2\,..\,\delta-\beta+s \times \&c.} - \frac{1}{1 \times 1\,.\,2\,.\,3\,..\,m-1} \times \frac{1}{\alpha-\beta-1\,.\,\alpha-\beta\,..\,\alpha-\beta+m-1}$$

$$\frac{1}{n-1\times\gamma-\beta-1\,.\,\gamma-\beta\,.\,.\,\gamma-\beta+r-1\times\delta-\beta-1\,.\,\delta-\beta\,.\,.\,\delta-\beta+s-1\times\&c.}+$$

$$\frac{1}{1\,.\,2\times1\,.\,2\,.\,3\,.\,.\,m-2}\times\frac{1}{\alpha-\beta-2\,.\,\alpha-\beta-1\,.\,\alpha-\beta\,.\,.\,\alpha-\beta+n-2\times\gamma-\beta-2}$$

$$\frac{1}{.\,\gamma-\beta-1\,.\,.\,\gamma-\beta+r-2\times\delta-\beta-2\,.\,\delta-\beta-1\,.\,.\,\delta-\beta+s-2\times\&c.}-$$

$$\frac{1}{1\,.\,2\,.\,3\times1\,.\,2\,.\,3\,.\,.\,m-3}\times\frac{1}{\alpha-\beta-3\times\alpha-\beta-2\times\alpha-\beta-1\,.\,.\,\alpha-\beta+n-3\times\gamma-}$$

$$\frac{1}{\beta-3\,.\,\gamma-\beta-2\,.\,\gamma-\beta-1\,.\,.\,\gamma-\beta+r-3\times\delta-\beta-3\,.\,.\,\delta-\beta+s-3\times\&c.}\;\ldots$$

$$\frac{1}{1\,.\,2\,.\,3\,.\,4\,.\,.\,m}\times\frac{1}{\alpha-\beta-m\,.\,\alpha-\beta-m+1\,.\,.\,\alpha-\beta+n-m\times\gamma-\beta-m\,.\,\gamma-\beta-}$$

$$\frac{1}{m+1\,.\,.\,\gamma-\beta-m+r\times\delta-\beta-m\,.\,\delta-\beta-m+1\,.\,.\,\delta-\beta-m+s\times\&c.}=0;$$

$$\frac{1}{1\,.\,2\,.\,3\,.\,.\,r}\times\frac{1}{\alpha-\gamma\,.\,\alpha-\gamma+1\,.\,.\,\alpha-\gamma+n\times\beta-\gamma\,.\,\beta-\gamma+1\,.\,\beta-\gamma+m}$$

$\frac{1}{\times\delta-\gamma\,.\,\delta-\gamma+1\,.\,.\,\delta-\gamma+s\times\&c.}-\&c.=0$, &c. $=0$; or, to explain it otherwiſe, aſſume the quantities α, $\alpha+1$, $\alpha+2$, $\alpha+3$, . . . $\alpha+n$; β, $\beta+1$, $\beta+2$, . . $\beta+m$; γ, $\gamma+1$, $\gamma+2$, . . $\gamma+r$; δ, $\delta+1$, $\delta+2$, . . $\delta+s$; &c. ſubtract one α from all the remaining quantities $\alpha+1$, $\alpha+2$, . $\alpha+n$; β, $\beta+1$, . . $\beta+m$; γ, $\gamma+1$, . . $\gamma+r$; δ, $\delta+1$, . . $\delta+s$; &c. and multiply all the differences reſulting 1, 2, 3, . . n; $\beta-\alpha$, $\beta-\alpha+1$, . . $\beta-\alpha+m$; $\gamma-\alpha$, $\gamma-\alpha+1$, . . $\gamma-\alpha+2$, . . $\gamma-\alpha+r$; $\delta-\alpha$, $\delta-\alpha+1$, . . $\delta-\alpha+s$, &c. into each other, and call the content p^{α}. In the ſame manner ſubtract $\alpha+1$ from all the remaining quantities α, $\alpha+2$, $\alpha+3$, . . $\alpha+n$; β, $\beta+1$, . . $\beta+m$; γ, $\gamma+1$, . . $\gamma+r$; δ, $\delta+1$, . . $\delta+s$; &c.; and let the remainders -1, 1, 2, 3, . . $n-1$; $\beta-\alpha-1$, $\beta-\alpha$, $\beta-\alpha+1$, . . $\beta-\alpha+m-1$; $\gamma-\alpha-1$, $\gamma-\alpha$, $\gamma-\alpha+1$, . . $\gamma-\alpha+r-1$; $\delta-\alpha-1$, $\delta-\alpha$, $\delta-\alpha+1$, . . $\delta-\alpha+s-1$, &c. be multiplied together, and their content be called $p^{\alpha+1}$. In the ſame manner ſubtract $\alpha+2$, $\alpha+3$, . . $\alpha+n$ reſpectively from all the remaining quantities, and let the differences reſulting be multiplied together, and their reſpective contents be called $p^{\alpha+2}$, $p^{\alpha+3}$, $p^{\alpha+4}$, .. $p^{\alpha+n}$; then, if the ſum of the ſeries can be found, will $\frac{1}{p\alpha}+\frac{1}{p\alpha+1}+\frac{1}{p\alpha+2}+\frac{1}{p\alpha+3}\ldots\frac{1}{p\alpha+n}=0$.

In the ſame manner ſubtract β, $\beta+1$, $\beta+2$, .. $\beta+m$ reſpectively from all the remaining quantities, and multiply their reſpective remainders into each other, and call their contents reſpectively p^{β}, $p^{\beta+1}$, $p^{\beta+2}$, .. $p^{\beta+m}$, then will $\frac{1}{p\beta}+\frac{1}{p\beta+1}+\frac{1}{p\beta+2}\ldots\frac{1}{p\beta+m}=0$.

Subtract γ, $\gamma+1$, $\gamma+2$, .. $\gamma+r$ reſpectively from all the remaining quantities, and multiply their reſpective remainders into each other, and call their contents reſpectively p^{γ}, $p^{\gamma+1}$, $p^{\gamma+2}$, .. $p^{\gamma+r}$; then will $\frac{1}{p\gamma}+\frac{1}{p\gamma+1}+\frac{1}{p\gamma+2}\ldots\frac{1}{p\gamma+r}=0$.

Subtract δ, $\delta+1$, $\delta+2$, .. $\delta+s$ from all the remaining quantities, and multiply their reſpective remainders into each other, and call their contents reſpectively p^{δ}, $p^{\delta+1}$, $p^{\delta+2}$, $p^{\delta+s}$; then will $\frac{1}{p\delta}+\frac{1}{p\delta+1}+\frac{1}{p\delta+2}+\frac{1}{p\delta+s}=0$; and ſo on; and, *vice verſâ*, if the ſum of the above-mentioned fractions be reſpectively $=0$; then the ſum of the ſeries, whoſe general term is the given one, can be found; otherwiſe not.

13. If the ſum of the ſeries, whoſe general term is

$$\frac{1}{z+\alpha\,.\,z+\alpha+1\,..\,z+\alpha+n\times z+\beta\,.\,z+\beta+1\,..\,z+\beta+m\times z+\gamma\,.\,z+\gamma+1\,..\,z+\gamma+r\times z+\delta\,.\,z+\delta+1\,..\,z+\delta+s\times\&c.=\mathrm{H}},$$

can be found in finite terms; then the ſum of a ſeries, whoſe general term is $\frac{az^{l}+bz^{l+1}+cz^{l+2}+\&c.}{\mathrm{H}}$ (where l denotes an affirmative number) can be alſo expreſſed in finite terms.

14. Let $A\times\overline{z+\beta}.\overline{z+\gamma}.\overline{z+\delta}\times\overline{z+\epsilon}\times\&c.+B\times\overline{z+\alpha}\times\overline{z+\gamma}.\overline{z+\delta}.\overline{z+\epsilon}.\&c.+C\times\overline{z+\alpha}.\overline{z+\beta}\times\overline{z+\delta}.\overline{z+\epsilon}.\&c.+D\times\overline{z+\alpha}.\overline{z+\beta}.\overline{z+\gamma}\times\overline{z+\epsilon}.\&c.+E\times\overline{z+\alpha}.\overline{z+\beta}.\overline{z+\gamma}.\overline{z+\delta}\times\&c.=1$, whatever may be the value of z; then will $A=\frac{1}{\beta-\alpha.\gamma-\alpha.\delta-\alpha.\&c.}$, $B=\frac{1}{\alpha-\beta.\gamma-\beta.\delta-\beta.\&c.}$, $C=\frac{1}{\alpha-\gamma.\beta-\gamma.\delta-\gamma.\&c.}$, $D=\frac{1}{\alpha-\delta.\beta-\delta.\gamma-\delta.\&c.}$, &c.

15. Let the general term of a ſeries be $\frac{1}{H}\times e^{z}$, where e is leſs than 1; the ſum of the ſeries can be expreſſed in finite terms, when the above-mentioned quantities $\frac{1}{p\alpha}+\frac{\frac{1}{e}}{p\alpha+1}+\frac{\frac{1}{e^2}}{p\alpha+2}+\frac{\frac{1}{e^3}}{p\alpha+3}\ldots+\frac{\frac{1}{e^n}}{p\alpha+n}=0$, $\frac{1}{p\beta}+\frac{\frac{1}{e}}{p\beta+1}+\frac{\frac{1}{e^2}}{p\beta+2}+\frac{\frac{1}{e^3}}{p\beta+3}\ldots+\frac{\frac{1}{e^m}}{p\beta+m}=0$, $\frac{1}{p\gamma}+\frac{\frac{1}{e}}{p\gamma+1}+\frac{\frac{1}{e^2}}{p\gamma+2}\ldots\frac{\frac{1}{e^r}}{p\gamma+r}=0$, $\frac{1}{p\delta}+\frac{\frac{1}{e}}{p\delta+1}+\frac{\frac{1}{e^2}}{p\delta+2}+\frac{\frac{1}{e^3}}{p\delta+3}\ldots\frac{\frac{1}{e^s}}{p\delta+s}=0$, &c. $=0$. This never happens unleſs $e=1$.

16. If the general term be any rational function of z into the exponential e^z, viz. $\frac{az^l+bz^{l-1}+cz^{l-2}+\&c.}{\overline{z+\alpha}^{m}\times\overline{z+\alpha+h}^{m'}\times\overline{z+\alpha+k}^{m''}\times\&c.\times\overline{z+\beta}^{n}\times\overline{z+\beta+h'}^{n'}\times\overline{z+\beta+k'}^{n''}\times\&c.\times\overline{z+\gamma}^{r}\times\overline{z+\gamma+h''}^{r'}\times\overline{z+\gamma+k''}^{r''}\times\&c.}\times e^{z}=K$, where h, h', h'', &c. k, k', k'', &c. l, m, m', m'', &c. n, n', n'', &c. r, r', r'', &c. &c. denote whole numbers, and neither

$\alpha-\beta$, nor $\alpha-\gamma$, nor $\beta-\gamma$, &c. are whole numbers: let m be the greateſt of the indices m, m', m'', &c.; n the greateſt of the indices n, n', n'', &c.; r the greateſt of the indices r, r', r'', &c.; then, from the ſums of the ſerieſes given, whoſe general terms are $\frac{e^z}{(z+\alpha)^m}, \frac{e^z}{(z+\alpha)^{m-1}}, \frac{e^z}{(z+\alpha)^{m-2}}, \ldots \frac{e^z}{(z+\alpha)^2}, \frac{e^z}{z+\alpha}$; $\frac{e^z}{(z+\beta)^n}, \frac{e^z}{(z+\beta)^{n-1}} \ldots \frac{e^z}{z+\beta}$; $\frac{e^z}{(z+\gamma)^r}, \frac{e^z}{(z+\gamma)^{r-1}}, \ldots \frac{e^z}{z+\gamma}$, &c. can be deduced the ſum of the above ſeries, whoſe general term is given above; multiply each of theſe terms into unknown co-efficients e', f, g, h, &c.; then reduce them to a common denominator, which is the ſame as the denominator of the given general term, and add them together, and make the correſpondent terms of the ſum reſulting equal to the correſpondent terms of the numerator $az^l+bz^{l-1}+cz^{l-2}+$&c. of the given general term; and from the equations reſulting can be deduced the co-efficients e', f, g, h, &c. and thence from the given ſums the ſum of the ſeries required.

Approximations to the ſums of the ſerieſes may be deduced from the methods given in the *Meditationes Analyticæ*. The ſum of ſome few caſes have been given from the periphery of the circle: for example, when α and m are whole numbers, and $e=1$; or, more particularly, when $m=2$ and $e=1$, and ſome other particular caſes, which may be with nearly the ſame facility calculated from approximations; the caſes given indeed are ſo few, unleſs when $e=1$, that they can very rarely be applied.

17. If the dimenſions of z in the numerator be equal or greater than its dimenſions in the denominator; that is, l be equal or greater than $m+m'+m''+$&c.$+n+n'+n''+$&c.$+r+r'+r''$,

$+$

+ &c. &c. reduce the fractions to a mixed number, ſo that the dimenſions of z in the numerator of the fraction be leſs than its dimenſions in the denominator, and the integral part be $Ae^{z} + Bze^{z} + Cz^{2}e^{z} + Dz^{3}e^{z} + \ldots Hz^{m}e^{z}$: the ſum of the infinite ſeries whoſe term is $Ae^{z} + Bze^{z} + Cz^{2}e^{z} + Dz^{3}e^{z} + Ez^{4}e^{z} + \ldots + Hz^{m}e^{z} = \frac{Ae}{1-e} + B\left(\frac{1}{(1-e)^{2}} - \frac{1}{1-e}\right) + Ce\left(\frac{1\cdot 2}{(1-e)^{3}} - \frac{1}{(1-e)^{2}}\right) + De\left(\frac{1\cdot 2\cdot 3}{(1-e)^{4}} - \frac{\overline{1+2}\times 1\cdot 2}{(1-e)^{3}} + \frac{1}{(1-e)^{2}}\right) + Ee\left(\frac{1\cdot 2\cdot 3\cdot 4}{(1-e)^{5}} - \&c.\right)$: the ſum of a ſeries (whoſe general term is $z^{m}e^{z}$) $= \frac{1\cdot 2\cdot 3\cdot 4\ldots m}{(1-e)^{m+1}}e - \frac{{}^{1}S^{m-1}}{(1-e)^{m}}1\cdot 2\cdot 3\ldots\overline{m-1}e + \frac{{}^{1}S^{m-1}\times{}^{1}S^{m-2} - {}^{2}S^{m-1}}{(1-e)^{m-1}}\times 1.2.3.\overline{m-2}.e \ldots + \frac{L}{(1-e)^{m-b}}\times 1\cdot 2\cdot 3\ldots\overline{m-b-1}\ldots \pm \frac{1}{(1-e)^{2}}$, where L is equal to the ſum of all quantities of the following ſort, ${}^{\alpha}S^{m-1}\times{}^{\beta}S^{m-\alpha-1}\times{}^{\gamma}S^{m-\alpha-\beta-1}\times{}^{\delta}S^{m-\alpha-\beta-\gamma-1}\times{}^{\epsilon}S^{m-\alpha-\beta-\gamma-\delta-1}\times$ &c. where α, β, γ, δ, &c. are whole affirmative numbers; (in the preceding notation by ${}^{\pi}S^{\rho}$ is deſigned the ſum of the contents of every π of the following numbers 1, 2, 3, 4, 5, . . . ρ); and $\alpha+\beta+\gamma+\delta+\epsilon+$ &c. $= b+1$; the above-mentioned product ${}^{\alpha}S^{m-1}\times{}^{\beta}S^{m-\alpha-1}\times$ &c. is to be taken affirmative or negative, according as the number of letters α, β, γ, δ, &c. is even or uneven.

The ſum of the ſeries $z^{m}\times e^{z}$ may alſo be found by aſſuming for it $(az^{m} + bz^{m-1} + cz^{m-2} + dz^{m-3}\ldots k)e^{z}$; then, finding its ſucceſſive term $(a\times\overline{z+1}^{m}e + b\overline{z+1}^{m-1}e + c\overline{z+1}^{m-2}e +$ &c.$)\ e^{z}$, and taking the difference between it and the aſſumed quantity, there reſults $(a\times\overline{e-1}\,z^{m} + \overline{mae+be-1}\,z^{m-1} +$ &c.$)\,e^{z}$; by equating it to the given term $z^{m}e^{z}$ are deduced the ſubſequent equations $a\times\overline{e-1} = 1$, $mae+be-1 = 0$, &c. whence

$a = \frac{1}{e-1}$, $b = \frac{1}{e} - ma$, &c.

If $e = 1$, then aſſume $az^{m+1} + bz^{m} +$ &c. for the ſum ſought, which rule was firſt taught by M. J. BERNOULLI.

If the term be $z^{m} f^{bz}$; for f^{b} ſubſtitute e, and there reſults $z^{m} e^{z}$ the ſame as before.

18. Let $P = A + Bx^{n} + Cx^{2n} + Dx^{3n} +$ &c. then will the ſum of the ſeries $\frac{A}{\alpha \,.\, \beta \,.\, \gamma \,.\, \delta \,.\, \&c.} + \frac{Bx^{n}}{\alpha+n \,.\, \beta+n \,.\, \gamma+n \,.\, \&c.} + \frac{Cx^{2n}}{\alpha+2n \,.\, \beta+2n \,.\, \gamma+2n \,.\, \&c.} + \&c. = \frac{1}{\alpha\beta\gamma\delta, \&c.} \times P - \frac{1}{\alpha} \,.\, \frac{1}{\beta-\alpha} \,.\, \frac{1}{\gamma-\alpha} \,.\, \frac{1}{\delta-\alpha} \,.\, \times \&c.\, x^{-\alpha} \int x^{\alpha} \dot{p} - \frac{1}{\beta} \,.\, \frac{1}{\alpha-\beta} \,.\, \frac{1}{\gamma-\beta} \,.\, \frac{1}{\delta-\beta} \,.\, \&c.\, x^{-\beta} \int x^{\beta} \dot{p} - \frac{1}{\gamma} \,.\, \frac{1}{\alpha-\gamma} \,.\, \frac{1}{\beta-\gamma} \,.\, \frac{1}{\delta-\gamma} \,.\, \&c. \times x^{-\gamma} \int x^{\gamma} \dot{p} - \&c.$

This may be proved from the ſubſequent arithmetical propoſition $\frac{1}{\alpha} \,.\, \frac{1}{\beta-\alpha} \,.\, \frac{1}{\gamma-\alpha} \,.\, \frac{1}{\delta-\alpha} \,.\, \&c. + \frac{1}{\beta} \,.\, \frac{1}{\alpha-\beta} \,.\, \frac{1}{\gamma-\beta} \,.\, \frac{1}{\delta-\beta} \,.\, \&c. + \frac{1}{\gamma} \,.\, \frac{1}{\alpha-\gamma} \,.\, \frac{1}{\beta-\gamma} \,.\, \frac{1}{\delta-\gamma} \,.\, \&c. + \frac{1}{\delta} \,.\, \frac{1}{\alpha-\delta} \,.\, \frac{1}{\beta-\delta} \,.\, \frac{1}{\gamma-\delta} \,.\, \&c. = \frac{1}{\alpha \,.\, \beta \,.\, \gamma \,.\, \delta \,.\, \&c.}$

19. Let the general term of the above-mentioned ſeries $A + Bx^{n} + Cx^{2n} +$ &c. be Hx^{zn}; then from the ſums of the ſeries p, and the fluents of the fluxions $x^{\alpha}\dot{p}$, $x^{\beta}\dot{p}$, $x^{\gamma}\dot{p}$, $x^{\delta}\dot{p}$, &c. being given, there follows the ſum of a ſeries, whoſe general term is $\frac{az^{ln} + bz^{ln-1} + cz^{ln-2} + \&c.}{\alpha+nz \,.\, \beta+nz \,.\, \gamma+nz \,.\, \delta+nz \,.\, \&c.} \times Hx^{zn}$, where l denotes a whole number.

If $H = m \,.\, \frac{m-1}{2} \,.\, \frac{m-2}{3} \ldots \frac{m-z}{z+1}$, or $= m \,.\, \frac{m-1}{2} \,.\, \frac{m-2}{3} \ldots \frac{m-lz}{lz+1}$, where l is a whole number, and m, α', β', γ', &c. are either whole numbers or fractions whoſe denominator is 2, and $\alpha = \alpha' n$, $\beta = \beta' n$, &c. the ſum of the above-mentioned ſeries can be found by finite terms, circular arcs and logarithms.

If $H = \frac{1}{1 \cdot 2 \cdot 3 \,.\,.\, z}$ or $= \frac{1}{1 \cdot 2 \cdot 3 \,.\,.\, lz}$, and l, α, β, γ, &c. whole numbers and $n = 1$; then can the ſum of any ſeries of the above-mentioned formula be found in finite algebraical functions of x, and the circular arcs and logarithms of them.

PART II.

1. THE doctrine of proportional parts was probably very early known in the æra of ſcience; for when men could not find the exact value of a quantity, they were induced to find near approximations by trials, and from thence, by proportion, an approximation ſtill nearer: which method is commonly denominated the Rule of Falſe.

This was often found to deviate conſiderably from the exact value; and the ſame operation was repeated, which frequently produced a nearer approximate value, and ſo on.

This method of approximations, the moſt general yet known, has been uſed in reſolving problems by ſeveral of the moſt eminent mathematicians in different ages, and in this particularly by M. EULER.

2. The following obſervation, I believe, was firſt publiſhed in the *Meditationes*, in the year 1770, *viz.* that the convergency of the approximate values, found by the rule of falſe and method of infinite ſeries, generally depended on this, *viz.*

how much nearer the approximate aſſumed is to one value of the quantity ſought poſſible or impoſſible than to any other, and not to the quantity itſelf: hence, when two or more (n) values of the quantity ſought are nearly equal, it is neceſſary to recur to more difficult rules, *viz.* to three or more trials; as, for example, let two roots be nearly equal, and write a, $a+\pi$, and $a+\varrho$, for the unknown quantity in the given equation made $=0$, and let the quantities reſulting be A, B, and C, then will more near approximations to the two roots nearly equal of the given equation be $a+$ the two roots (x) of the quadratic $\left(\frac{A}{\pi\rho}+\frac{B}{\pi(\pi-\rho)}+\frac{C}{\rho(\rho-\pi)}\right)x^2-\left(A\times\frac{\pi+\rho}{\pi\rho}+B\times\frac{\rho}{\pi\cdot(\pi-\rho)}+\frac{C\pi}{\rho\cdot(\rho-\pi)}\right)x+A=0$: for write 0, π, and ρ, reſpectively for x in the equation, and there will reſult the quantities A, B, and C.

More generally, ſubſtitute for x in the given equation the quantities a, $a+\pi$, $a+\rho$, $a+\sigma$, $a+\tau$, $a+\upsilon$, &c. where π, ϱ, σ, τ, &c. are very ſmall quantities; and let the quantities reſulting be A, B, C, D, E, F, &c.; then will more near approximations to the (n) roots of the given equation be $a+\alpha$, $a+\beta$, $a+\gamma$, $a+\delta$, &c. where α, β, γ, δ, &c. &c. are the n roots (e) of the given equation $\frac{(e-\pi)(e-\varrho)(e-\sigma)(e-\tau)\&c.}{\pm\pi\varrho\sigma\tau,\ \&c.}\times A+\frac{e(e-\varrho)(e-\sigma)(e-\tau)\&c.}{\pi(\pi-\varrho)(\pi-\sigma)(\pi-\tau)\&c.}\times B+\frac{e(e-\pi)(e-\sigma)(e-\tau)\&c.}{\varrho(e-\pi)(\varrho-\sigma)(\varrho-\tau)\&c.}\times C+\&c.=A-\left(\frac{A}{\pi}-\frac{B}{\pi}\right)e+\left(\frac{A}{\pi\varrho}+\frac{B}{\pi(\pi-\varrho)}+\frac{C}{\varrho(\varrho-\pi)}\right)\times e\cdot\overline{e-\pi}-\left(\frac{A}{\pi\varrho\sigma}-\frac{B}{\pi(\pi-\varrho)(\pi-\sigma)}-\frac{C}{\rho(\rho-\pi)(\rho-\sigma)}-\frac{D}{\sigma(\sigma-\pi)(\sigma-\varrho)}\right)\times e\cdot\overline{e-\pi}\cdot\overline{e-\rho}+\left(\frac{A}{\pi\rho\sigma\tau}+\frac{B}{\pi(\pi-\varrho)(\pi-\sigma)(\pi-\tau)}+\frac{C}{\varrho(\varrho-\pi)(\varrho-\sigma)(\varrho-\tau)\&c.}+\frac{D}{\sigma(\sigma-\pi)(\sigma-\varrho)(\sigma-\tau)\&c.}+\frac{E}{\tau(\tau-\pi)(\tau-\varrho)(\tau-\sigma)\&c.}\right)\cdot\overline{e-\pi}\cdot\overline{e-\varrho}\cdot\overline{e-\sigma}-\bigg(\frac{A}{\pi\varrho\sigma\tau\upsilon}-\frac{B}{\pi(\pi-\varrho)(\pi-\sigma)(\pi-\tau)(\pi-\upsilon)}-\frac{C}{\varrho(\varrho-\pi)(\varrho-\sigma)(\varrho-\tau)}$

$$\frac{}{(\rho-\tau)(\rho-\upsilon)} - \frac{D}{\sigma(\sigma-\pi)(\sigma-\rho)(\sigma-\tau)(\sigma-\upsilon)} - \frac{E}{\tau(\tau-\pi)(\tau-\rho)(\tau-\sigma)(\tau-\upsilon)} - \frac{F}{\upsilon(\upsilon-\pi)(\upsilon-\rho)(\upsilon-\sigma)(\upsilon-\tau)}\Big) e \,.\, \overline{e-\pi} \,.\, \overline{e-\rho} \,.\, \overline{e-\sigma} \,.\, \overline{e-\tau} + \&c. = 0.$$

Their reſolutions were firſt given in the *Meditationes Analyticæ*, publiſhed in the year 1774, and require the extraction of a quadratic, cubic, and in general of an equation of (n) dimenſions; which rules will often give a nearer approximate than the preceding, when the roots are not nearly equal.

3. Theſe rules may be applied to find approximations to the roots of algebraical equations: for example, let the algebraical equation be $x^n - px^{n-1} + qx^{n-2} - \&c. = 0$, ſubſtitute in it for x two quantities a, and $a + e$ much nearer to one root than to any other, and there reſult $a^n - pa^{n-1} + qa^{n-2} - \&c = A$, and $(a+e)^n - p(a+e)^{n-1} + q(a+e)^{n-2} - \&c. = B$; then, by the rule of falſe, $B - A : e :: A : \dfrac{a^n - pa^{n-1} + qa^{n-2} - \&c.}{(na^{n-1} - \overline{n-1}pa^{n-2} + \overline{n-2}qa^{n-3} - \&c.) + \&c.} = b$; whence $a - b$ a near approximate value to the root ſought. If the quantities, in which are involved e, e^2, e^3, &c. on account of e being very ſmall, be rejected, then will the approximate ſought $b = \dfrac{a^n - pa^{n-1} + qa^{n-2} - \&c.}{na^{n-1} - \overline{n-1}pa^{n-2} + \overline{n-2}qa^{n-3} - \&c.}$; which will nearly be the ſame as found, where a near approximate is given, from the method given by **VIETA**, **HARRIOT**, **OUGHTRED**, **NEWTON**, **DE LAGNY**, **HALLEY**, &c.

4. From this expreſſion it follows, that if (a) be a root of the equation $na^{n-1} - \overline{n-1}pa^{n-2} + \&c. = 0$, of which the roots are limits between the roots of the given equation, the approximation found will be infinite.

5. In finding theſe approximations, when there are two or more quantities contained in the given equation dependent on

each other, as the arc and the ſine, it is neceſſary that both ſhould be corrected in every approximation to ſuch a degree as the ſubſequent approximations require.

6. In the *Meditationes* it is obſerved, that in any algebraical equation $x^n - ax^{n-1} + bx^{n-2} - cx^{n-3} + dx^{n-4} - ex^{n-5} .. \pm gx^{n-m+1} \mp hx^{n-m} \pm kx^{n-m-1} \mp lx^{n-m-2} \pm$ &c. $= 0$, if a be much greater than $\frac{b}{a}$, and $\frac{b}{a}$ has to $\frac{c}{b}$ a much greater ratio than $a : \frac{b}{a}$; and in the ſame manner $\frac{c}{b}$ has to $\frac{d}{c}$ a much greater ratio than $\frac{b}{a} : \frac{c}{b}$, and ſo on; then will a be a near approximate to the greateſt root of the algebraical equation; $\frac{b}{a}$ a near approximate to the ſecond; $\frac{c}{b}$ a near approximate to the third, and $\frac{k}{h}$ a near approximate to a root, which is much leſs than m roots of the given equation, but much greater than the remaining $(n - m - 1)$ roots.

If the equation above-mentioned $x^n \pm ax^{n-1} + \&c. = 0$, or which is the ſame, $1 \pm \frac{a}{x} + \frac{b}{x^2} \pm \&c. = 0$ be infinite; then will, in like manner, all its roots be poſſible and their approximate values a, $\frac{b}{a}$, $\frac{c}{b}$, &c. as before.

This eaſily appears by ſubſtituting for a, b, c; &c. their values in terms of the root of the equation.

7. A nearer approximate to the above-mentioned root will be $\frac{k}{h} - \left(\frac{l}{k} - \frac{gk^2}{h^3}\right) + \&c.$

8. Equations, of which the fluxions of the quantities contained in the given equations can be found, may be reduced to infinite algebraical equations, in which is involved no irrational function of the unknown quantities contained in the given equations by the incremental theorem; *viz.* let $A = 0$ be the given

equation, and (a) an approximate much more near to the root required (π) of the given equation than to any other: write a for (x) the unknown quantity ſought in the ſubſequent quantities, A, $\frac{\dot{A}}{\dot{x}}$, $\frac{\ddot{A}}{\dot{x}^2}$, $\frac{\dddot{A}}{\dot{x}^3}$, &c.; and let there reſult the correſpondent quantities α, β, γ, δ, &c.; then will $\pi - a$ be a root (e) of the infinite equation $\alpha \pm \beta e + \frac{1}{1 \cdot 2} \gamma e^2 \pm \frac{1}{1 \cdot 2 \cdot 3} \delta e^3 + \frac{1}{1 \cdot 2 \cdot 3 \cdot 4} \varepsilon e^4 \pm \&c. = 0$, of which a root of the equations $\alpha \pm \beta e = 0$, $\alpha \pm \beta e + \frac{1}{1 \cdot 2} \gamma e^2 = 0$, &c. will be an approximate. If two roots of the given equation are nearly $= a$, then it is neceſſary to recur to an equation not inferior to a quadratic.

9. The ſucceſſive approximate values found by theſe and like rules will ultimately be to each other in a greater than any geometrical ratio: for example, let $\frac{as}{a+s}$ be an approximate to a root of the quadratic $x^2 - (a+s)x + as = 0$, then will the new addition to the approximate to the root s found by the common method at the diſtance $n-1$ from the firſt approximate be $\frac{s^b}{a^{b-1}}$ nearly, where $b = 2^{n-1}$.

10. Let an equation $x^n - Px^{n-1} + Qx^{n-2} - Rx^{n-3} + Sx^{n-4} - \&c. = 0$, of which the roots are a, b, c, d, &c.; and an equation $x^n - px^{n-1} + qx^{n-2} - rx^{n-3} + sx^{n-4} - \&c. = 0$, where p, q, r, s, &c. differ from the co-efficients P, Q, R, S, &c. by very ſmall quantities: aſſume the (n) equations $\pi + \rho + \sigma + \tau + \&c. = p - P = \alpha$, $a\pi + b\rho + c\sigma + d\tau + \&c. = P\alpha - q + Q = \beta$, $a^2\pi + b^2\rho + c^2\sigma + d^2\tau + \&c. = P\beta - Q\alpha + r - R = \gamma$, $a^3\pi + b^3\rho + c^3\sigma + d^3\tau + \&c. = P\gamma - Q\beta + R\alpha - s + S = \delta$, &c.; and from them find the unknown quantities π, ρ, σ, τ, &c; then will $a + \pi$, $b + \rho$, $c + \sigma$, $d + \tau$, &c. be nearly the n roots of the equation $x^n - px^{n-1} + qx^{n-2}$

− &c. = 0, and conſequently π, ρ, &c. nearly $-\frac{a^n - pa^{n-1} + qa^{n-2} - \&c.}{na^{n-1} - \overline{n-1}pa^{n-2} + \&c.}$, $-\frac{b^n - pb^{n-1} + qb^{n-2} - \&c.}{nb^{n-1} - \overline{n-1}pb^{n-2} + \&c.}$, &c.; whence the convergency of the approximate values found by this rule depends on the principle before delivered.

10. Let there be given (m) equations, which contain m unknown quantities x, y, z, &c.; and let α, β, γ, &c. be nearly correſpondent values of the unknown quantities x, y, &c. reſpectively: aſſume $n + 1$ different values of the quantity x, viz. α, $\alpha + \pi$, $\alpha + \pi'$, $\alpha + \pi''$, &c. &c.; and in like manner aſſume $n + 1$ different correſpondent values of the quantity y, which let be β, $\beta + \rho$, $\beta + \rho'$, $\beta + \rho''$, &c.; and ſo of the remaining; where π, π', π'', &c. ρ, ρ', ρ'', &c. &c. are very ſmall quantities; ſubſtitute theſe quantities for their reſpective values in the given equations, and let the reſulting quantities be A, B, C, D, &c. in the firſt equation; P, Q, R, S, &c. in the ſecond, &c. &c.: aſſume from the firſt equation the n ſimple equations $a\pi + b\rho + \&c. = B - A$, $a\pi' + b\rho' + \&c. = C - A$, $a\pi'' + b\rho'' + \&c. = D - A$, &c.; and from the ſecond equation the n ſimple ones $h\pi + k\rho + \&c. = Q - P$, $h\pi' + k\rho' + \&c. = R - P$, $h\pi'' + k\rho'' + \&c. = S - P$, &c. From theſe equations can be inveſtigated the co-efficients a, b, &c. h, k, &c. &c.; ultimately aſſume the m equations $A + ae + bi + \&c. = 0$, $P + he + ki + \&c. = 0$, &c. from which can be deduced the values of the quantities e, i, &c.; and $\alpha + e$, $\beta + i$, &c. will be more near values of the quantities, x, y, &c.

11. Sir ISAAC NEWTON found the ſum (A) of the $2n$th power of each of the roots of a given equation, and then extracted the $2n$th root of A, *viz.* $\sqrt[2n]{A}$ for an approximate value of the greateſt root of the equation, and further added ſome ſimilar rules on the ſame principle.

In the *Miſcell. Analyt.* and *Meditationes* the ſame principle is applied in different rules for finding approximates to the greateſt and other roots of the given equation; and alſo limits of the ratios of the approximate values of the roots found by theſe rules to the roots themſelves are given.

It is obſerved in the *Meditationes*, that from theſe rules in general to find the greateſt root, it is often neceſſary that the greateſt poſſible root be greater than the ſum of the quantities contained in the poſſible and impoſſible part of any impoſſible root of the given equation: for example, if $a+b\sqrt{-1}$ be an impoſſible root of the given equation, then it is neceſſary that the greateſt poſſible root be greater than $a+b$.

It may further be obſerved, that in equations of high dimenſions (unleſs purpoſedly made) it is probable, the number of impoſſible will greatly exceed the number of poſſible roots; and conſequently theſe rules moſt commonly fail.

12. M. Bernoulli aſſumed a fraction whoſe numerator is a rational function of the unknown quantity, and denominator the quantity, which conſtitutes the equation; and reduced the fraction into a ſeries, whoſe terms proceed according to the dimenſions of the unknown quantity; and thence found an approximate value of the greateſt or leaſt root of the given equation or its reciprocal, by dividing the co-efficient of any term of the ſeries reſulting by the co-efficient of the preceding or ſubſequent term: for example, let the equation be $x^n-px^{n-1}+qx^{n-2}-rx^{n-3}+sx^{n-4}\,..\pm Px^3\mp Qx^2\pm Rx\mp S=0$; aſſume the fraction $\frac{nx^{n-1}-\overline{n-1}px^{n-2}+\overline{n-2}qx^{n-3}-\overline{n-3}rx^{n-4}+\&c.}{x^n-px^{n-1}+qx^{n-2}-rx^{n-3}+\&c.}=$ $nx^{n-1}+(\alpha+\beta+\gamma+\delta+\&c.)x^{n-2}+(\alpha^2+\beta^2+\gamma^2+\delta^2+\epsilon^2+\&c.)x^{n-3}+(\alpha^3+\beta^3+\gamma^3+\delta^3+\epsilon^3+\&c.)x^{n-4}+(\alpha^4+\beta^4+\gamma^4+\delta^4+\epsilon^4+\&c.)$

&c.)$x^{-5}\ldots+(\alpha^{m-1}+\beta^{m-1}+\gamma^{m-1}+$&c.$=P)x^{-m}+(\alpha^{m}+\beta^{m}+\gamma$ + &c. $=Q)\,x^{-m-1}+(\alpha^{m+1}+\beta^{m+1}+\gamma^{m+1}+$&c.$=R)\,x^{-m-2}+$ &c.; then will $\frac{R}{Q}$ or $\sqrt[m]{Q}$ be the greateſt root nearly.

Ex. 2. $\frac{R-2Qx+3Px^{2}-\text{\&c.}\ldots nx^{n-1}}{S-Rx+Qx^{2}-Px^{3}+\text{\&c.}\ldots x^{n}}=\left(\frac{1}{\alpha}+\frac{1}{\beta}+\frac{1}{\gamma}+\text{\&c.}\right)+\left(\frac{1}{\alpha^{2}}+\frac{1}{\beta^{2}}+\frac{1}{\gamma^{2}}+\frac{1}{\delta^{2}}+\text{\&c.}\right)x+\left(\frac{1}{\alpha^{3}}+\frac{1}{\beta^{3}}+\frac{1}{\gamma^{3}}+\text{\&c.}\right)x^{2}+\left(\frac{1}{\alpha^{4}}+\frac{1}{\beta^{4}}+\frac{1}{\gamma^{4}}+\text{\&c.}\right)x^{3}+\ldots\ldots+\left(\frac{1}{\alpha^{m-1}}+\frac{1}{\beta^{m-1}}+\frac{1}{\gamma^{m-1}}+\text{\&c.}=P\right)x^{m-2}+\left(\frac{1}{\alpha^{m}}+\frac{1}{\beta^{m}}+\frac{1}{\gamma^{m}}+\text{\&c.}=Q\right)x^{m-1}+\left(\frac{1}{\alpha^{m+1}}+\frac{1}{\beta^{m+1}}+\frac{1}{\gamma^{m+1}}+\text{\&c.}\right)x^{m}+$ &c.; then will $\frac{P}{Q}$ or $\sqrt[m]{\frac{1}{Q}}$, the leaſt root nearly.

Ex. 3. $\frac{1}{x^{n}-px^{n-1}+qx^{n-2}-\text{\&c.}}=x^{-n}+(\alpha+\beta+\gamma+\delta+\text{\&c.})x^{-n-1}+(\alpha^{2}+\beta^{2}+\gamma^{2}+\text{\&c.}(+\alpha\beta+\alpha\gamma+\beta\gamma+\alpha\delta+\text{\&c.}))\,x^{-n-2}+(\alpha^{3}+\beta^{3}+\gamma^{3}+\text{\&c.}(+\alpha^{2}\beta+\alpha^{2}\gamma+\beta^{2}\gamma+\gamma^{2}\alpha+\gamma^{2}\beta+\text{\&c.})+\alpha\beta\gamma+\alpha\beta\delta+\alpha\gamma\delta+\beta\gamma\delta+\text{\&c.}))\,x^{-n-3}+$&c.; and $\frac{S}{S-Rx+Qx^{2}-\text{\&c.}\ldots x^{n}}=1+\left(\frac{1}{\alpha}+\frac{1}{\beta}+\frac{1}{\gamma}+\text{\&c.}\right)x+\left(\frac{1}{\alpha^{2}}+\frac{1}{\beta^{2}}+\frac{1}{\gamma^{2}}+\text{\&c.}\left(+\frac{1}{\alpha\beta}+\frac{1}{\alpha\gamma}+\frac{1}{\beta\gamma}+\text{\&c.}\right)\right)x^{2}+$&c. in each of which all the numeral co-efficients are 1. The approximate values to the greateſt and leaſt root may be found in the ſame manner as before.

From the preceding examples it appears, that the ſame obſervations which have been applied to Sir ISAAC NEWTON's method are equally applicable to M. BERNOULLI's.

13. In the *Meditationes* this rule is further extended, *viz.* let the given equation involve irrational and other functions of the unknown quantity; reduce it ſo that no function of the unknown quantity (x) may be contained in the denominator, and let the reſulting equation be $A=0$. Aſſume a fraction

$\frac{B}{A}$, whoſe numerator B is a finite rational and integral function of the unknown quantity; reduce $\frac{B}{A}$ into a ſeries proceeding according to the dimenſions of the unknown quantity: for example, let the ſeries be $A'x^{r}+B'x^{r+s}+Cx^{r+2s}+\ldots+Lx^{r+hs}+Mx^{r+\overline{h+1}s}+Nx^{r+\overline{h+2}s}+$ &c.; then *(exceptis excipiendis)* if s be negative, will the greateſt root be $\sqrt[s]{\frac{M}{L}}$ nearly; but, if s be affirmative, $\sqrt[s]{\frac{L}{M}}$ will be the leaſt root nearly. If l be infinite, then *(exceptis excipiendis*, as before-mentioned) the quantities $\sqrt[s]{\frac{M}{L}}$ and $\sqrt[s]{\frac{L}{M}}$ will be the above-mentioned roots accurately.

Theſe principles have been applied to find the remaining roots of the given equation as well as the greateſt and leaſt.

14. The rule of falſe has been found very uſeful in finding approximates to the two unknown quantities contained in two given equations, and has been applied to (n) equations having (n) different unknown quantities: for example, it has been obſerved, that if two or more (m) values of an unknown quantity (x) are nearly equal to each other and to its given approximate value (x'), the unknown quantity $v=x-x'$ will aſcend to two or more (m) dimenſions in one of the reſulting equations; or in more than one equations will be contained ſuch powers of the quantity (v), that if the more equations were reduced to one whoſe unknown quantity is v, the reſulting equation will contain (m) dimenſions of the quantity v. Hence it appears, that in this caſe alſo the convergency of the approximate values found will depend on the given approximate being much more near to one root than to any other.

15. When the given equations $A=0$, $B=0$, $C=0$, &c. contain irrational or other quantities whoſe fluxions can be found; and approximates (a, b, c, d, &c.) are given to each of the unknown quantities (x, y, z, v, &c.) contained in the given equations; let $a+x'=x$, $b+y'=y$, $c+z'=z$, $d+v'=v$, &c.: ſubſtitute in the quantities A, $\left(\frac{A^{\cdot}}{\dot{x}}\right)$, $\left(\frac{A^{\cdot}}{\dot{y}}\right)$, $\left(\frac{A^{\cdot}}{\dot{z}}\right)$, $\left(\frac{A^{\cdot}}{\dot{v}}\right)$, &c.; $\left(\frac{\ddot{A}}{\dot{x}^2}\right)$, $\left(\frac{\ddot{A}}{\dot{x}\dot{y}}\right)$, $\left(\frac{\ddot{A}}{\dot{x}\dot{z}}\right)$, $\left(\frac{\ddot{A}}{\dot{x}\dot{v}}\right)$, &c.; $\left(\frac{\dddot{A}}{\dot{x}\dot{y}\dot{z}}\right)$, $\left(\frac{\dddot{A}}{\dot{x}^2\dot{y}}\right)$, &c. for x, y, z, v, &c. reſpectively a, b, c, d, &c.; let the reſulting correſpondent values be A', $\left(\frac{{}'A^{\cdot}}{\dot{x}}\right)$, $\left(\frac{{}'A^{\cdot}}{\dot{y}}\right)$, &c. whence may be deduced the equation $A'+\left(\left(\frac{{}'A^{\cdot}}{\dot{x}}\right)x'+\left(\frac{{}'A^{\cdot}}{\dot{y}}\right)y'+\left(\frac{{}'A^{\cdot}}{\dot{z}}\right)z'+\left(\frac{{}'A^{\cdot}}{\dot{v}}\right)v'+\text{\&c.}\right)+\left(\frac{1}{2}\left(\frac{{}'A^{\cdot\cdot}}{\dot{x}^2}\right)x'^2+\frac{1}{2}\left(\frac{{}'A^{\cdot\cdot}}{\dot{y}^2}\right)y'^2+\frac{1}{2}\left(\frac{{}'A^{\cdot\cdot}}{\dot{z}^2}\right)z'^2+\text{\&c.}+\left(\frac{{}'A^{\cdot}}{\dot{x}\dot{y}}\right)x'y'+\left(\frac{{}'A^{\cdot\cdot}}{\dot{x}\dot{z}}\right)x'z'+\text{\&c.}\right)+\text{\&c.}=0$ in which no irrational, &c. function of the approximates x', y', z', &c. is contained; and in the ſame manner may the remaining equations $B=0$, $C=0$, &c. be transformed into others, in which no irrational function of the approximates (x', y', z', &c.) is contained, and from the reſulting equations may be found approximate values of the quantities x', y', z', &c.

If there be given only two equations $A=0$ and $B=0$ containing two unknown quanties x and y, and all the quantities of the reſulting equations, in which are contained more than one dimenſion of the quantities x' and y' be rejected, there will reſult the two equations $A'+\left(\frac{A'^{\cdot}}{\dot{x}}\right)x'+\left(\frac{A'^{\cdot}}{\dot{y}}\right)y'=0$ and $B'+\left(\frac{B'^{\cdot}}{\dot{x}}\right)x'+\left(\frac{B'}{\dot{y}}\right)y'=0$, from which may be found x' and y', the ſame as given by Mr. **Simpson** and others. When two or more values of the quantity x are nearly $=a$, then in a reſulting equation or equations, quantities of two or more dimenſions of the approximate x' are to be included.

PART III.

1. THE firſt algebraiſts divided quantities, and extracted their roots no further than the quantities themſelves: they did not perceive the utility of proceeding any further, otherwiſe the operation would have been the ſame continued. Mr. **Gregory St. Vincent**, and Mr. **Mercator** divided, and Sir **Isaac Newton** divided and extracted the roots of quantities (in which only one unknown quantity (x) is contained) by the operations then uſed by arithmeticians, into ſeries aſcending or deſcending, according to the dimenſions of x *in infinitum*. They clearly ſaw the utility of it in finding the fluents of fluxions, as Dr. **Wallis** and others ſome little time before had found the fluent of the fluxion $ax^{\frac{m}{n}}\dot{x}$; or, which is the ſame, the area of a curve whoſe ordinate is $ax^{\frac{m}{n}}$ and abſciſſa is x.

2. M. **Leibnitz** aſked from Mr. **Newton** the caſes in which the above-mentioned ſerieſes would converge; for it would be altogether uſeleſs when it diverges, and of little uſe when it converges ſlowly.

Caſ. 1. To this queſtion an anſwer, I believe, was firſt given in the *Meditationes*, viz. reduce the function to its loweſt terms; and alſo in ſuch a manner that the quantities contained in the numerator and denominator may have no denominator: make the denominator $Q=0$, and every diſtinct irrational quantity contained in it $=0$; and alſo every diſtinct irrational quantity H contained in the numerator $=0$; then, let α be the leaſt root affirmative or negative (but not $=0$) of the above-mentioned reſulting equations, the aſcending ſeries will always converge, if the value of x is contained between α and $-\alpha$; but if x be greater than α or $-\alpha$, the above-mentioned ſeries will not converge.

If the above-mentioned ſeries (S) be multiplied into $\dot{x}$, and its fluent found; then will the ſeries denoting the fluent contained between two values a and b of the quantity (x) converge, when a and b are both contained between α and $-\alpha$: the fluent always converges faſter than the ſeries S, the unknown quantity x having the ſame values in both.

Ex. Let $\int\frac{\dot{x}}{a+bx+cx^2+\,..\,x^n}=Ax+\frac{1}{2}Bx^2+\frac{1}{3}Cx^3+$ &c. and the roots of the equation $a+bx+cx^2+\,..\,x^n=0$ be α, β, γ, δ, &c.; then $\frac{\dot{x}}{a+bx+cx^2\,..\,x^n}=\frac{\pi\dot{x}}{\alpha-x}+\frac{\rho\dot{x}}{\beta-x}+\frac{\sigma\dot{x}}{\gamma-x}+\text{\&c.}=A\dot{x}+Bx\dot{x}+Cx^2\dot{x}+\text{\&c.}$; but $\frac{\pi}{\alpha-x}=\frac{\pi}{\alpha}+\frac{\pi x}{\alpha^2}+\frac{\pi x^2}{\alpha^3}+$ &c. *in infinitum*, $\frac{\rho}{\beta-x}=\frac{\rho}{\beta}+\frac{\rho x}{\beta^2}+\frac{\rho x^2}{\beta^3}+$ &c. &c.; the former ſeries converges when x is contained between α and $-\alpha$, the latter when x is between β and β, and ſo on. In the ſame manner the fluents $\int\frac{\pi\dot{x}}{\alpha-x}=\frac{\pi}{\alpha}x+\frac{\pi x^2}{2\alpha^2}+$ &c.; $\int\frac{\rho\dot{x}}{\beta-x}=\frac{\rho x}{\beta}+\frac{\rho x^2}{2\beta^2}+$ &c. &c. *a fortiori* converge when x is between α and $-\alpha$, β and $-\beta$, &c. reſpectively, and ſo on: hence the ſeries $Ax+\frac{1}{2}Bx^2+\frac{1}{3}Cx^3+$ &c.

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$=\int\frac{\dot{x}}{a+bx+cx^2\ldots x^n}=\int\frac{\pi\dot{x}}{\alpha-x}+\int\frac{\varrho\dot{x}}{\beta-x}+\int\frac{\sigma\dot{x}}{\gamma-x}+\&c.=\left(\frac{\pi}{\alpha}+\frac{\varrho}{\beta}+\frac{\sigma}{\gamma}+\&c.\right)x+\left(\frac{\pi}{\alpha^2}+\frac{\varrho}{\beta^2}+\frac{\sigma}{\gamma^2}+\&c.\right)x^2+\&c.$ always converges when x is between α and $-\alpha$, where α is the leaſt root of the above-mentioned equation; but where x is greater than α or $-\alpha$, the ſeries will diverge.

The infinite ſeries $a^m+ma^{m-1}x+m.\frac{m-1}{2}a^{m-2}x^2+\&c.=\overline{a+x}^m$ will always converge when a is greater than x, and diverge when leſs, and conſequently its convergency does not depend on the index m, unleſs when $x=\pm a$: and in the *Meditationes Analyticæ* are given the caſes in which it converges or diverges when $\mp a=x$; and alſo if the ſeries $x^m+max^{m-1}+\&c.=\overline{x+a}^m$ deſcends according to the dimenſions of x, when it converges or diverges.

Caſ. 2. Let the above-mentioned quantity be reduced into a ſeries $Ax^{-r}+Bx^{-r-1}+\&c.$ deſcending according to the dimenſions of the unknown quantity x; then will the ſeries $Ax^{-r}+Bx^{-r-1}+\&c.=P$, or the ſeries $\frac{Ax^{-r+1}}{1-r}-\frac{Bx^{-r}}{r}+\&c.=\int P\dot{x}$ converge, when x is greater than the greateſt root (λ) of the above-mentioned equations, and diverge when it is leſs; and conſequently in this caſe, when the fluent is required between the two values a and b of x; the ſeries found will converge when a and b are both greater than λ.

Caſ. 3. When x is equal to the leaſt root in the former caſe, and to the greateſt in the latter, then ſometimes the ſeries will converge, and ſometimes not. Theſe different caſes are given in the *Meditationes*; but it would be too long to inſert them in this Paper.

4. If any roots are impoſſible as $a-b\sqrt{-1}$ and $a+b\sqrt{-1}$, then the ſeries will converge when x is in the firſt caſe

leſs than $\pm(a-b)$, and in the ſecond caſe greater than $\pm(a+b)$; or, more general, it will converge in the firſt caſe when x^n is always infinitely leſs than the reciprocal of the quantity $\frac{(a+b\sqrt{-1})^n+(a-b\sqrt{-1})^n=P}{(a^2+b^2)^n}$, where n is infinite; and in the latter caſe it will converge when x^n is infinitely greater than P.

It may not be improper to obſerve, that the ſame values of the root are to be applied in the equations, which are applied in the ſeries.

3. Sir ISAAC NEWTON, in the binomial theorem, reduced the power or root of a binomial into a ſeries proceeding according to the dimenſions of the terms contained in the binomial. M. DE MOIVRE reduced the power or root of a multinomial into a like ſeries; but in all caſes, except the moſt ſimple, we muſt ſtill recur to the common diviſion, extraction of roots, &c.

4. Meſſ. EULER, MACLAURIN, and other mathematicians, finding that the ſerieſes before-mentioned often converged ſlowly, or, if the truth may be confeſſed, commonly not at all, to deduce the area of a curve contained between two values a and b of the abſciſs, or fluent of a fluxion between two values a and b of the variable quantity x, interpolated the ſeries or area, between a and b; that is, found the area or fluent contained between the abſciſſæ a and $a+\alpha$, then between the abſciſſæ $a+\alpha$ and $a+2\alpha$, and then between the abſciſſæ $a+2\alpha$ and $a+3\alpha$, and ſo on, till they came to the area between $b-\alpha$ and b. M. EULER obſerved, that when the ordinate became 0 or infinite, the ſeries expreſſing the area converges ſlowly; and therefore,

in order to inveſtigate the area near the points of the abſciſs; where the ordinates become o or infinite, he transforms the equation, and finds serieſes expreſſing the area near thoſe points; in which ſerieſes the abſciſſæ or unknown quantities begin from thoſe points.

5. In the *Meditationes* it is aſſerted, that in a ſeries proceeding according to the dimenſions of x, if any root of the above-mentioned equations be ſituated between the beginning of the abſciſs o and its end x, the ſeries will not converge: it is therefore neceſſary to transform the abſciſs ſo that it may begin or end at each of the roots of the above-mentioned equations, and conſequently where the ordinates become o or infinite, &c.; thoſe caſes excepted where the ordinate becomes o, and its correſpondent abſciſſa is a root of a rational function W of x without a denominator, and $\int W P\dot{x}$ is equal to the given ſeries; and in general the abſciſſæ ought to begin from the above-mentioned points; for if they end there the ſeries will converge very ſlow, if at all.

6. It is further aſſerted, that if a and b, the values of the abſciſſæ between which the area is required, be both more near to one root of the above-mentioned equations than to any other, and n ſerieſes are to be found, whoſe ſum expreſs the area contained between a and b; then that the ſum of the (n) ſerieſes may converge the ſwifteſt, the diſtances of the beginnings of each of the n abſciſſæ from the adjacent root will form a geometrical progreſſion.

7. Mr. CRAIG found the fluent of any fluxion of the formula $(a + bx^{n} + cx^{2n} + \&c.)^{m}x^{\theta-1}\dot{x}$ by a ſeries of the following

kind $(a+bx^n+cx^{2n}+\&c.)^{m+1} \times x^\theta \times (\alpha+\beta x^n+\gamma x^{2n}+\&c.$ *in infinitum)*. Sir ISAAC NEWTON, by ſerieſes of the ſame kind, found the fluents of fluxions of this formula $(a+bx^n+cx^{2n}+\&c.)^l \times (e+fx^n+gx^{2n}+\&c.)^m \times \&c.\ x^{\theta-1}\dot{x}$; the ſame principle is extended ſomewhat more general in the *Meditationes*.

8. Mr. JOHN BERNOULLI found the fluent of any fluxion $\int n\dot{z} = nz - \frac{z^2\dot{n}}{2\dot{z}} + \frac{z^3\dot{n}}{2\cdot 3\dot{z}^2} - \&c.$ from the principles which Mr. CRAIG publiſhed for finding the fluents of fluxions involving fluents.

In the *Meditationes* ſomething is added of the convergency of theſe ſeries; and alſo,

9. In them a new method is given of finding approximations. Let ſome terms in the given quantity be much leſs or greater than the reſt; then reduce the quantity into terms proceeding according to the dimenſions of the ſmall quantities, or according to the reciprocals of the great quantities, and it is done. If the fluent of the quantity reſulting is required, find it from the common methods, if poſſible; but if not, reduce the terms not to be found into an infinite ſeries, and then find approximate values to each of the terms, &c.

Ex. 1. Let R the radius, and A the arc of a circle whoſe ſine is S and coſine C, and $A \pm e$ an arc of a circle which does not much differ from the arc A, that is, let e be a very ſmall quantity: then will the ſine of the arc $A \pm e$ be $S(1 - \frac{1}{2} \times \frac{e^2}{R^2} + \frac{1}{2\cdot 3\cdot 4} \times \frac{e^4}{R^4} - \frac{1}{2\cdot 3\cdot 4\cdot 5\cdot 6} \times \frac{e^6}{R^6} + \&c.) \pm C(\frac{e}{R} - \frac{1}{2\cdot 3} \times \frac{e^3}{R^3} + \frac{1}{2\cdot 3\cdot 4\cdot 5}\ \frac{e^5}{R^5} - \&c.)$; and the coſine of the ſame arc will be $C(1 - \frac{1}{2} \times \frac{e^2}{R^2} + \frac{1}{2\cdot 3\cdot 4} \times \frac{e^4}{R^4} - \&c.$ *in infinitum*$) \pm S(\frac{e}{R} -$

$\frac{1}{2\cdot 3}\times\frac{e^3}{R^3}+\frac{1}{2\cdot 3\cdot 4\cdot 5}\times\frac{e^5}{R^5}-$ &c.).

Ex. 2. Log. $(x\pm e)=\int\frac{\dot{x}}{x\pm e}=\log. x\pm\frac{e}{x}\mp\frac{e^2}{2x^2}\pm\frac{e^3}{3x^3}\mp$ &c.:

$\int\frac{\dot{x}}{a^2-(x+e)^2}=\int\frac{\dot{x}}{a^2-x^2}+\frac{e}{a^2-x^2}+$ &c.

Ex. 3. $\int\frac{\dot{x}}{a^2+(x-e)^2}=\int\frac{\dot{x}}{a^2+x^2}-\frac{e}{a^2+x^2}+$ &c.

Ex. 4. $\int\frac{\dot{y}}{\sqrt{(1-(y\pm e)^2)}}=\int\frac{\dot{y}}{\sqrt{1-y^2}}\pm\frac{e}{\sqrt{1-y^2}}\pm$ &c.

Ex. 5. $\int\frac{\dot{y}}{\sqrt{1-y^2}+e}=\int\frac{\dot{y}}{\sqrt{1-y^2}}-\int\frac{e\dot{y}}{1-y^2}+\int\frac{e^2\dot{y}}{(1-y^2)^{1\frac{1}{2}}}-$ $\int\frac{e^3\dot{y}}{(1-y^2)^2}+$ &c.

Ex. 6. Let the fluxion be $\frac{\sqrt{1-cx^2}\,\dot{x}}{\sqrt{1-x^2}}$, where c is a very ſmall quantity; then, if P be put for $\sqrt{1-x^2}$, the fluxion becomes $\frac{\dot{x}}{P}-\frac{cx^2\dot{x}}{2P}-\frac{c^2x^4\dot{x}}{8P}-\frac{c^3x^6\dot{x}}{16P}-$ &c. of which the fluents will be found $A-\frac{c}{2}\times\frac{1\,.\,A-xP}{2}-\frac{c^2}{8}\times\frac{3B-x^3P}{4}-\frac{c^3}{16}\times\frac{5C-x^5P}{6}-$ &c. where $A=\int\frac{\dot{x}}{P}$, $B=\frac{1\,.\,A-xP}{2}$, $C=\frac{3B-x^3P}{4}$, &c.

This is the ſwifteſt converging ſeries for finding the length of the arc of an ellipſe nearly circular, which is yet known; for example, let the abſciſs to the axis beginning from the center $= x$, the ſemi-tranſverſe axis of the ellipſe be 1, its ſemi-conjugate $1-d$; then will $c=2d-d^2$, and let the length of the quadrant of the ellipſe be required, in this caſe $x=1$, and $P=\sqrt{1-x^2}=0$; and $A=\frac{3,14159,\text{ \&c.}}{2}=1,57079$, &c. whence the length required is 1,57079, &c. $\times\left(1-\frac{c}{2\,.\,2}-\frac{1\cdot 3c^2}{2^2\cdot 4^2}-\frac{1\cdot 3^2\cdot 5c^3}{2^2\cdot 4^2\cdot 6^2}-\frac{1\cdot 3^2\cdot 5^2\cdot 7c^4}{2^2\cdot 4^2\cdot 6^2\cdot 8^2}-\text{\&c.}\right)$.

Ex. 7. Let the fluxion be $\dot{x}(a^3 + a' + (3a^2b + b')x + (3ab^2 + c')x^2 + (b^3 + d')x^3)^m = P\dot{x}$, where a', b', c', &c. are very ſmall quantities; then will $P\dot{x} = ((a+bx)^{3m} + m\overline{a+bx}^{3m-3} \times (a' + b'x + c'x^2 + d'x^3) + m.\frac{m-1}{2}\,\overline{a+bx}^{3m-6} \times (a' + b'x + c'x^2 + d'x^3)^2 + \&c.)\,\dot{x}$, of which the fluent is $\frac{1}{3m+1\,.\,b}\,\overline{a+bx}^{3m+1} + m\,(a+bx)^{3m-1} \times \left(\frac{d'}{(3m+1)b}x^3 + \frac{c'-3aA}{3mb}x^2 + \frac{b'-2aB}{(3m-1)b}x + \frac{a'-aC}{(3m-2)b}\right)$ + &c. where the letters A, B, C, &c. denote the preceding co-efficients.

10. M. EULER and others, reduced the ſeries $Ax^r + Bx^{r+s} + Cx^{r+2s}$ + &c. into a ſeries A′ ſin. $r\alpha$ + B ſin. $\overline{r+s}\,\alpha$ + &c. &c. where α denotes the arc of a circle, whoſe ſine is ax, &c. It may be eaſily reduced into infinite other ſerieſes proceeding according to the dimenſions of quantities, which are functions of x; but it is moſt commonly preferable to reduce it into ſerieſes proceeding according to the ſines, coſines, tangents, or ſecants of the arcs of circle, which ſines, &c. can immediately be procured from the common tables.

It has been obſerved in the firſt part, that to find the root of an equation, an approximate value much more near to one root of the equation than to any other muſt be given. In this part it is further obſerved, that ſerieſes deduced from expanding given quantities, ſo as to proceed according to the dimenſions of the unknown or variable quantities, will not converge if the unknown quantities be greater than the leaſt roots of the abovementioned equations; and that they will not converge much, unleſs the unknown quantities have a ſmall proportion to the leaſt roots; and if the given quantities be expanded into ſerieſes deſcending according to the dimenſions of the unknown quantities, then the ſerieſes reſulting will not converge if the greateſt

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greateſt roots of the equations before-mentioned be greater than the unknown quantities; and unleſs the unknown quantities have a great ratio to the greateſt roots the ſerieſes will converge ſlowly: for example, the ſerieſes $\int \frac{\dot{x}}{1+x} = x - \frac{1}{2}x^2 + \&c.$, $\int \frac{\dot{z}}{1+z^2} = z - \frac{1}{3}z^3 + \frac{1}{5}z^5 - \&c.$, $\int \frac{\dot{y}}{\sqrt{1-y^2}} = y + \frac{1}{6}y^3 + \&c.$ will never converge if x, z, or y, be greater than 1; but will always converge when leſs than ± 1 or $\pm 1\sqrt{-1}$ the leaſt or only roots of the equations $1+x=0$, $1-y^2=0$, and $1+z^2=0$. The ſeries $y + \frac{1}{6}y^3 + \&c.$ will always converge when y is ſituated between $+1$ and -1, in which caſe alone it is poſſible. The ſame is true alſo of a ſeries ariſing from expanding the $\int (ax^m + bx^{m-1} + cx^{m-2} + \&c.)^r \dot{x}$ into a ſeries proceeding according to the dimenſions of x, if the equation $ax^m + bx^{m-1} + cx^{m-2} + \&c. = 0$ has only two poſſible roots α and $-\alpha$, which are leſs in the manner before-mentioned than any impoſſible root contained in it.

If in either of the above-mentioned ſerieſes the unknown quantity x, z, or y, has a great proportion to 1, the ſeries will converge very ſlow; for example, if $x=1$, ten thouſand numbers at leaſt are to be calculated, to procure the ſum of the ſeries true to four figures; therefore, in theſe and moſt other ſerieſes it is neceſſary firſt to find a near value, *viz.* when x either $= z$, when e is very ſmall; or $= e$, when z is very ſmall; and then write $z+e$ for x in the quantity, and reduce it in the former caſe into a ſeries proceeding according to the dimenſions of e, in the latter caſe according to the dimenſions of z, and there will ariſe two ſerieſes, of which the fluents properly corrected, *viz.* by adding the fluent contained between the values a and e to the latter, and that between a and z to the former, will give the ſame fluent. The firſt term of the ſeries, in which e is ſuppoſed very ſmall, will be the fluent of the given fluxion, when $x=z$.

11. If a fluxion $P\dot{x}$, where P is a function of x, be transformed into another $Q\dot{z}$, where Q is a function of z, and they be reduced into ſerieſes A and B, proceeding according to the dimenſions of x and z reſpectively; find α and π, correſpondent values of the quantities x and z; then in aſcending ſerieſes, if α has a leſs ratio to the leaſt root of the equation $P=0$, than π has to the leaſt root of the equation $Q=0$, the ſeries A *(exceptis excipiendis)* will converge ſwifter than the ſeries B.

12. Dr. BARROW, in ſome equations, expreſſing the relation between the abſciſs x and ordinate y, found y in the two firſt terms of x, viz. $y=a+bx$, which is an equation to the aſymptotes of the curves. Sir ISAAC NEWTON, from an algebraical equation given, expreſſing the relation between y and x, found a ſeries proceeding according to the dimenſions of x, expreſſing y in terms of x. M. LEIBNITZ performed the ſame problem by aſſuming a ſeries $Ax^n+Bx^{n+r}+Cx^{n+2r}+\&c.$ with general co-efficients, and ſubſtituting this ſeries for y in the given equation, &c. from equating the correſpondent terms he deduced the indexes and co-efficients. M. DE MOIVRE, Mr. MAC LAURIN, &c. obſerved, that when the higheſt terms of the given equations have two or more (m) diviſors equal; for example, $(y-ax^n)^m$; to which we muſt add, and when a value of y in this caſe is required nearly equal to Ax^n, a ſeries $Ax^n+Bx^{n+\frac{r}{m}}+\&c.$ is to be aſſumed, whoſe indexes differ only by $\frac{r}{m}$, &c. if otherwiſe they would differ by r.

This reduction ſeldom anſwers any other purpoſe than finding the fluents of fluxions as $\int y\dot{x}$, &c.; of the aſymptotes, &c.

of curves, which depend on ſome of the firſt terms of the ſeries; but will very ſeldom be uſed for finding the roots of an equation; the rule of falſe, or method given by VIETA, will ever be ſubſtituted in its ſtead.

13. The values of x may be required between which the above-mentioned ſeries $Ax^n+Bx^{n+r}+Cx^{n+2r}+$&c. will converge, as the infinite ſeries anſwers no purpoſe when it diverges. Firſt, if an aſcending be required, write for y in the given algebraical equation an infinite quantity, and find the roots of x in the equation thence reſulting $P=0$; which for y write in the ſame equation, and find the roots of x in the reſulting equation which contain irrational quantities, *viz.* if one root be $x=a$; then let it contain $(x-a)^m$, where m is not a whole number; find the roots of the equations reſulting from making every irrational function of (x) contained in the given equation $=0$, there being no irrational function of y contained in it; then, if x be greater than the leaſt root not $=0$ of the above-mentioned equations, the ſeries will not converge; but if it be a ſeries deſcending according to the dimenſions of x, and x be leſs than the greateſt root of the above-mentioned equations, the ſeries will not converge.

In interpolating ſerieſes to inveſtigate the fluent contained between two values a and b of the fluxion $(Ax^n+Bx^{n+r}+$&c.$)t\dot{x}$, it is preferable to make the abſciſſæ begin from every one of the above-mentioned roots contained between a and b.

Moſt commonly theſe ſerieſes will not converge unleſs x be leſs, &c. than other quantities not inveſtigated by this rule.

14. Sir ISAAC NEWTON gave an elegant example of this rule in the reverſion of the ſeries, $y=ax+bx^2+cx^3+$&c. from which the inveſtigation of the law of the ſeries has never been attempted. In the year 1757 I ſent the firſt edition of my

Meditationes Algebraicæ to the Royal Society, and publiſhed it in 1760, and afterwards in 1762, with another part added, on the Properties of curves, under the title of *Miſcellanea Analytica*, in all which was given the law of a ſeries for finding the ſum of the powers of the roots of an equation from its co-efficients. That great mathematician M. LE GRANGE and myſelf printed about the ſame time an obſervation known to me at the time that I printed the above-mentioned book, that the law of its powers and roots, if it proceeds *in infinitum*, is the ſame; from which ſeries of mine, with great ſagacity, M. LE GRANGE found the law which Sir ISAAC NEWTON's reverſion of ſeries obſerves. In the *Meditationes* the law is given, and the ſeries is made to proceed according to the dimenſions of x, &c.

15. It is aſſerted in the *Meditationes*, that in moſt equations of high dimenſions, unleſs purpoſedly conſtituted, the ſum of the terms which, from the given hypotheſis, become the greateſt, being ſuppoſed $=0$, only an approximate to the value Ax^n of y in the reſulting equation can by the common algebra be deduced. In this caſe the approximate to the quantity A is to be found ſo near as the approximate value of the quantity ſought requires; or perhaps it is preferable to correct in every operation the approximate values of the quantities A, B, C, &c. in the ſeries required $A'x^n + B'x^{n+r} + C'x^{n+2r} +$ &c.

In the equation the quantity $z \pm e$ may be ſubſtituted for x, and from the equation reſulting a ſeries expreſſing the value of y may be found, proceeding either according to the dimenſions of the quantity z, or its reciprocals, according to the conditions of the problem.

16. If there are more than one (n) equations having ($n+1$) unknown quantities (x, y, z, &c.): in each of the equations for y,

z, &c. write reſpectively Ax^n, $A'x^m$, &c.; and ſuppoſe the terms of each of the equations, which reſult the greateſt from the given or aſſumed hypotheſis $= 0$, and from the reſulting equations may be found the firſt approximates Ax^n, $A'x^m$, &c. either accurately or nearly; then, in the given equations for y, z, &c. write $y' + (A + a)\, x^n + Bx^{n+n'} + \&c.\, z' + (A' + a')\, x^m + B'x^{m+m'}$, where a, a', &c. are very ſmall quantities; and ſuppoſe the terms of each of the equations which become greateſt from the above-mentioned hypotheſis reſpectively $= 0$, and from the equations reſulting deduce the quantities a, a', &c.; n', m', &c.; B, B', &c.; and ſo on: or aſſume $y = (A + 1a + a1 + \&c.)\, x^n + (B + 1b + b1 + \&c.)\, x^{n+n1} + \&c.$; $z = (A' + 1a' + a'1 + \&c.)x^m + (B' + 1b' + b'1 + \&c.)x^{m+m1} + \&c.$ &c.; ſubſtitute theſe quantities for their values in the given equations, and from equating the correſpondent terms of the reſulting equations may be deduced the quantities required.

The differences of the indexes n', &c. m', &c. may be deduced by writing x^n, x^m, &c. for y, z, &c. in the given equations, from the differences of the indexes of the quantities reſulting. The ſame principles may be applied in finding the above-mentioned differences, when two or more values are Ax^n, &c. which were applied in a like caſe to one equation having two unknown quantities.

The ſame principles which diſcover the caſes in which a ſeries deduced from an equation having two unknown quantities will converge, may be applied for the ſame purpoſe to theſe ſeries.

17. In the equations for x, y, z, &c. write reſpectively $x' + e$, $y' + f$, $z' + g$, &c.; and from the equations reſulting find y', z', &c. in ſerieſes either proceeding according to the dimenſions

of the quantities e, f, g, &c.; or according to the dimenſions of the quantity x', as the conditions of the problem require.

18. Given one or more (n) algebraical equations involving ($n+m$) unknown quantities, one unknown quantity (y) may be expreſſed by a ſeries proceeding according to the dimenſions of the $m-1$ remaining ones (x, z, v, &c.), in which any dimenſions of z, v, &c aſſumed may be ſuppoſed to correſpond to (l) dimenſions of the quantity (x).

19. In a fluxional equation of the m^{th} order, expreſſing the relation between x, y, and their fluxions, where $\dot{x}$ is conſtant, Mr. EULER ſubſtitutes in the given equation for $\dot{y}^{m}$, $\dot{y}^{m-1}$, $\dot{y}^{m-2}$, $\dot{y}^{m-3}$, &c. reſpectively $Ax^{n-m}\dot{x}^{m}$, $\frac{A}{n-m+1}x^{n-m+1}\dot{x}^{m-1}+a\dot{x}^{m-1}$, $\frac{A}{(n-m+1)(n-m+2)}x^{n-m+2}\dot{x}^{m-2}+ax\dot{x}^{m-2}+b\dot{x}^{m-2}$, $\frac{A}{(n-m+1)(n-m+2)(n-m+3)}x^{n-m+3}\dot{x}^{m-3}+\frac{1}{2}ax^{2}\dot{x}^{m-3}+bx\dot{x}^{m-3}+c\dot{x}^{m-3}$, &c. where a, b, c, &c. are any quantities to be aſſumed in ſuch a manner as the conditions of the problem require; from ſuppoſing the aggregate of the terms of the reſulting equation, which are the greateſt, $=0$, may be deduced the firſt approximate Ax^{n}, or elſe (as is beforementioned) $A'x^{n}$ a near approximate to Ax^{n}, and by proceeding as in algebraical equations another approximate may be found, and ſo on. The ſame may be found by aſſuming $y = Ax^{n}+Bx^{n+r}+Cx^{n+2r}+$ &c. $+ax^{m}+bx^{m-1}+cx^{m-2}+$ &c. or $y = (A+1a+a1+$ &c.$)x^{n}+(B+1b+b1+$ &c.$)x^{n+r}+(C+1c+c1+$ &c.$)x^{n+2r}+$ &c. $+ax^{m}+bx^{m-1}+cx^{m-2}+$ &c. and ſubſtituting it and its fluxions for their values $y, \dot{y}, \ddot{y}$, &c. in the given equation, and ſuppoſing the aggregate of each correſpondent terms, which do not very much differ from each other, $=0$; from the reſulting equations can be deduced the co-efficients A, B, C, &c.; or A, $1a$, $a1$, &c.; B, $1b$, $b1$, &c.; C, $1c$, $c1$, &c. &c.

In the given equation for y, x, and their fluxions ſubſtitute $y'+f$, $x'+g$, and their fluxions, where the quantities f and g, &c. are aſſumed, ſo as to render the quantities y' and x', &c. very ſmall.

In finding the ſeries which expreſſes the value of y in terms of x, there will always occur as many invariable quantities to be aſſumed at will as is the order of the fluxional equation, provided the ſeries begins from its firſt terms; and to find them there will reſult equations eaſily reducible to homogeneous fluxional equations, of which the orders do not exceed m.

V. *Experiments on Hepatic Air.*

By Richard Kirwan, *Eſq. F. R. S.*

Read December 22, 1785.

HEPATIC Air is that ſpecies of permanently elaſtic fluid which is obtained from combinations of ſulphur with various ſubſtances, as alkalies, earths, metals, &c. It poſſeſſes many peculiar and diſtinct properties; among which the moſt obvious are, a diſagreeable characteriſtic ſmell emitted by no other known ſubſtance; inflammability, when mixed with a certain proportion of reſpirable or nitrous air; miſcibility with water, to a certain degree; and a power of diſcolouring metals, particularly ſilver and mercury. Theſe properties were firſt diſcovered by that incomparable analyſt M. Scheele.

This air acts an important part in the œconomy of nature. It is frequently found in coal-pits; and the truly excellent and ever to be regretted M. Bergman has ſhewn it to be the principle on which the ſulphureous properties of many mineral waters depend, and thus happily terminated the numerous diſputes which the obſcurity of that ſubject had occaſioned. There is alſo great reaſon to think, that it is the peculiar product of the putrefaction of many, if not all, animal ſubſtances. Rotten eggs and corrupt water are known to emit the ſmell peculiar to this ſpecies of air, and alſo to diſcolour metallic ſubſtances in the ſame manner. M. VIELLARD has lately diſcovered ſeveral other indications of this air in putrefied blood.

Yet, deſerving as this ſubſtance appears to be of a thorough inveſtigation, it has as yet been very little attended to. The experiments of M. BERGMAN have not been ſufficiently numerous, and thereby led him into ſome miſtakes. Dr. PRIESTLEY has intirely overlooked it. The reſearches of the ingenious M. SENNEBIER, of Geneva, have indeed been very extenſive; but as, for particular reaſons, he operated on this air over water (by which it is in great meaſure abſorbed) inſtead of quickſilver, his concluſions appear in many reſpects objectionable, as will be ſeen in the ſequel. The experiments I have now the honour of laying before the Society were all made over quickſilver, and ſeveral times repeated.

SECTION I.

Of the Subſtances that yield Hepatic Air, and the means of obtaining it.

It is well known, that *ſaline* liver of ſulphur is formed, in the dry way, of a mixture of equal parts of vegetable or mineral alkali and flowers of ſulphur, melted together by a moderate heat, in a covered crucible. I examined the circumſtances of its formation, and obſerved, that when this mixture was ſlightly heated, it emitted a bluiſh ſmoke, which gradually grew whiter as the heat was increaſed, and at laſt, when the mixture was perfectly melted, and the bottom of the crucible ſlightly red, became perfectly white and inflammable. To examine the nature of this ſmoke, I made a pretty pure fixed alkali, by deflagrating equal parts of cream of tartar and nitre in a red-hot crucible in the uſual way; and mixing this ſalt perfectly dry with flowers of ſulphur in much ſmaller quantity, as I believe (for I did not weigh the ſalt, leaſt it ſhould, during the weighing, attract moiſture) I gradually heated the mixture in a ſmall coated glaſs retort, and received the air proceeding from it over quickſilver.

The firſt portion of air that paſſed with a very gentle heat was that of the retort itſelf, ſlightly phlogiſticated. It amounted to 1,5 cubic inches, and with Dr. PRIESTLEY's nitrous teſt (that is, an equal meaſure of nitrous air) its goodneſs was 1,29. It contained no fixed air.

The ſecond portion of air obtained by increaſing the heat amounted to about 18 cubic inches. It was of a reddiſh colour, and ſeemed a mixture of nitrous and common air. It ſlightly acted on the mercury.

The third portion conſiſted of 20 cubic inches, and appeared to be of the ſame kind as the laſt, but mixed with a little fixed air.

This was ſucceeded by 64 cubic inches of almoſt perfectly pure fixed air; and the bottom of the retort being now red, ſome ſulphur ſublimed in its neck. When all was cold, liver of ſulphur was found in the bulb of the retort.

Hence we ſee, that the blue ſmoke conſiſts chiefly of fixed air, and the white or yellow ſmoke of ſulphur ſublimed; and that no hepatic air is thus formed; nor vitriolic air, unleſs the retort be ſo large as to contain a ſufficiency of common air to admit the combuſtion of part of the ſulphur.

2dly, That the aërial or any other acid, combined with the alkali, muſt be expelled before the alkali will combine with the ſulphur. Liver of ſulphur exerciſes a ſtrong ſolving power on the earth of crucibles, and readily pierces through them.

The above experiment ſeems to ſhew that liver of ſulphur will not yield hepatic air without the addition of an acid; and I believe this to be true when the experiment is made in the dry way, and nearly ſo in the moiſt way; for having added 200 grains of ſulphur to a concentrated ſolution of ſtrong cauſtic vegetable alkali, by a ſtrong and long-continued heat I obtained only 1 cubic inch of hepatic air; yet it is well known, that a ſtrong ſolution of liver of ſulphur conſtantly emits an hepatic ſmell, even in the temperature of the atmoſphere; and the ſubſtance ſo emitted contains ſo much hepatic air as to diſcolour ſilver and lead, and even their ſolutions; which ſhews, that an incomparably ſmall quantity of this air is capable of producing this effect. To diſcover whether this extrication of hepatic air might be cauſed by the depoſition of fixed air from the atmoſphere, I threw ſome pulveriſed calcareous hepar into aërated water, and by the application of heat endeavoured to obtain hepatic air, but in vain: and, indeed, the very circumſtance that the hepatic ſmell, and its effects, are always ſtrongeſt the firſt inſtant that a bottle of the hepatic ſolution is opened, ſeems to indicate that fixed air is no way concerned in its production.

The beſt liver of ſulphur is made of equal parts of ſalt of tartar and ſulphur; but as about one-fifth of the ſalt of tartar conſiſts of air which eſcapes during the operation, it ſeems, that the proportion of ſulphur predominates in the reſulting compound; yet as ſome of the ſulphur alſo ſublimes and burns, it is not eaſy to fix the exact proportion. 100 grains of the beſt, that is, the reddeſt liver of ſulphur, afford, with dilute marine acid, about 40 cubic inches of hepatic air, in the temperature of 60°: a quantity equivalent to about 13 grains of ſulphur, as will be ſeen in the ſequel.

The *marine* acid is the beſt adapted to the production of hepatic air. If the concentrated *nitrous* acid be uſed, it will afford nitrous air; but having diluted ſome nitrous acid, whoſe ſpecific gravity was 1,347, with 20 times its bulk of water, I obtained, with the aſſiſtance of heat, as pure hepatic air as with any other acid.

The concentrated *vitriolic* acid, poured on liver of ſulphur, affords but little hepatic air without the aſſiſtance of heat, though it inſtantly decompoſes the liver of ſulphur; and it is partly for this reaſon that the proportion of air is ſo ſmall; for it is during the *gradual* decompoſition of ſulphureous compounds that hepatic air is produced.

Diſtilled *vinegar* extricates this air in the temperature of the atmoſphere; but it is not pure, its peculiar ſmell being mixed with that of vinegar.

The *acid of ſugar* alſo produced ſome quantity of this air in the temperature of 59°.

20 grains of *ſedative ſalt*, or acid (as it ſhould more properly be called) diſſolved in an ounce of water, being poured on liver of ſulphur, afforded hepatic air only when in a boiling heat, or nearly ſo.

Neither the *aërial* nor *arſenical* acids produce any.

Liver of ſulphur is ſoluble not only in water but in ſpirit of wine, and in cauſtic volatile alkalies; and the colour of both ſolutions is red. Sulphur is precipitable from the former by the addition of water or of an acid, but from the latter only by an acid.

Having made ſome liver of ſulphur, in which the proportion of ſulphur much exceeded that of the alkali, I poured on part of it ſome oil of vitriol, whoſe ſpecific gravity was 1,863: by this means I obtained hepatic air, ſo loaded with ſulphur, that it depoſited ſome in the tube through which it was tranſmitted, and on the upper part of the glaſs receiver. This air I transferred to another receiver; but though it was then perfectly clear and tranſparent, and amounted to 6 cubic inches, yet the next morning the inſide of the glaſs was thickly lined with ſulphur, and the air reduced to 1 cubic inch, which was pure vitriolic air. Hence it appears, firſt, that a ſpecies of elaſtic fluid may exiſt in a ſtate intermediate between the aërial and the vaporous, which is not permanently elaſtic like air, nor immediately condenſed by cold like vapour, but which, by the gradual loſs of its ſpecific heat, may be reduced to a concrete form. 2dly, That ſo large a quantity of ſulphur may be combined with vitriolic air, as to enable it to exhibit the properties of hepatic air, for ſome time at leaſt. A mixture of three parts pulveriſed quick-lime and one part ſulphur, heated to whiteneſs in a covered crucible for one hour, became of a ſtony hardneſs, and being treated with marine acid, afforded hepatic air. If a piece of this ſtone be heated in pure water it becomes bluiſh, and hence the origin of the blue marles generally found near hot ſulphurated waters.

A calcareous hepar may alſo be formed in the moiſt way, as is well known.

Magneſia free from fixed air, heated in the ſame manner with ſulphur, afforded no hepatic air when an acid was poured on it.

I alſo procured this air from a mixture of three parts *filings of iron* and one of ſulphur, melted together, and treated with marine acid. It is remarkable, that this ſulphurated iron, diſſolved in marine acid, affords ſcarce any inflammable, but moſtly hepatic air.

A mixture of equal parts filings of iron and ſulphur, made into a paſte with water, after heating and becoming black, afforded hepatic air when an acid was poured on it; but this hepatic air was mixed with inflammable air, which probably proceeded from the uncombined iron. After a few days, this paſte loſt its power of producing hepatic air.

M. BERGMAN has remarked, that combinations of ſulphur with ſome other metals yield hepatic air.

I attempted extracting air from a mixture of oil of olives with cauſtic vegetable alkali. It immediately whitened, and on applying heat efferveſced ſo violently as to paſs over into the receiver: nor had I better ſucceſs on adding an acid, as I might well foreſee. The event was different when on a few grains of ſulphur I poured ſome of the oil, and heated them in a phial with a bent tube; for as ſoon as the ſulphur melted, the oil began to act on it, grew red, and emitted hepatic air, ſimilar to that produced by other proceſſes.

I alſo obtained this air in great plenty from a mixture of equal parts ſulphur and *pulveriſed charcoal*, out of which its adventitious air had been as much as poſſible expelled by keeping it a long time heated to redneſs, in a crucible on which a cover was luted, with a ſmall perforation to permit the air to eſcape. This air was inflammable, as appeared by holding a lighted candle before it during its emiſſion; yet it is hardly poſſible to free charcoal wholly from foreign air, for it ſoon re-attracts it when expoſed to the atmoſphere.

This laſt mixture, when diſtilled, affording a large quantity of hepatic and ſome inflammable air, without the addition of any acid, I imagined, that as the retort was only half full, it might contain a ſufficiency of atmoſpheric air to admit the combuſtion of part of the ſulphur, and thus furniſh the neceſſary acid; but when I filled the retort with air phlogiſticated by the nitrous teſt unto 1,8, and in this air diſtilled the above mixture, the reſult was exactly the ſame as when the retort was filled with atmoſpheric air.

Six grains of *pyrophorus*, made of alum and ſugar, efferveſced with marine acid, and afforded 2,5 cubic inches of hepatic air. This pyrophorus had been made ſix years before, and was kept in a tube hermetically ſealed, and for many ſummers expoſed to the ſtrongeſt light of the ſun. It was ſo combuſtible, that ſome grains of it took fire while it was introduced into the phial out of which the hepatic air was expelled.

A mixture of two parts *white ſugar* (previouſly melted in order to free it from water) with one part ſulphur, when heated to about 600 or 700 degrees, gave out hepatic air very rapidly. This air had a ſmell much reſembling that of onions; it contained no fixed air, nor ſaccharine, or other acid. But ſugar and ſulphur, melted together, gave out no hepatic air when treated with acids. Water, ſpirit of wine, and marine acid, decompoſe this mixture, diſſolving the ſugar, and leaving the ſulphur.

A mixture of ſulphur and *plumbago* afforded me no hepatic air.

I then tried whether ſulphur could combine with elaſtic fluids, and the reſults were as follows.

12 grains of ſulphur, heated in a retort, filled with metallic *inflammable* air, afforded no hepatic air; though when the retort was cold, and for ſome time expoſed to the air, it ſmelled of hepatic air. It is true, the heat I applied might be inſufficient; for the inflammable air paſſing over with a ſlight heat, the mercury aſcended ſo high into the neck of the receiver, that, fearing the rupture of the retort, I was obliged to interrupt

the operation. I had no better ſucceſs when the ſulphur, previous to its diſtillation, had been moiſtened with marine acid.

Again, I expoſed 18 grains of liver of ſulphur to ſix cubic inches of *fixed air*, thermometer 70°, for four days. The liver of ſulphur was ſomewhat whitened on the ſurface; the air had not an hepatic ſmell, but rather that of bread. It was not converted into phlogiſticated air, but ſeemed to have taken up ſome ſulphur, which lime-water ſeparated. It was not in the leaſt diminiſhed, and therefore ſeems to have received an addition of hepatic air, or rather of ſulphur.

I alſo expoſed a quantity of *ſulphureo-martial paſte* to fixed air, for five days. The fixed air was not diminiſhed, but received a ſlight acceſſion of inflammable air. The paſte itſelf, taken out of this air, and expoſed to the atmoſphere, heated ſtrongly.

Laſtly, I expoſed 3 grains of ſulphur to about 12 cubic inches of *marine air*. It was not diminiſhed in four days; nor was the ſulphur ſenſibly. On adding one cubic inch of water to this air, it was abſorbed all to one inch, and this had an hepatic ſmell; ſo that neither was the ſulphur decompoſed, nor the marine acid converted into inflammable air. The water had alſo an hepatic ſmell, and evidently contained ſulphur; for it precipitated the ſolution of ſilver *brown* mixed with white, and the nitrous ſolution of copper *reddiſh brown*, and when vegetable fixed alkali was dropped into it, let fall a white precipitate, namely, the ſulphur.

SECTION II.

Of the general Characters of Hepatic Air.

I found the abſolute weight of this air by weighing it in a glaſs bottle, previouſly exhauſted by Mr. HURTER's new improved air-pump, whoſe effect is ſo conſiderable as to leave in general only $\frac{1}{100}$ and frequently but $\frac{1}{1000}$ part of unexhauſted air. This bottle contained 116 cubic inches nearly; and this quantity of hepatic air weighed 38,58 grains, the thermometer being then 67°,5, the barometer 29,94, and M. SAUSSURE's hygrometer 84°, the weight of 116 cubic inches of atmoſpheric air being at the ſame time 34,87 grains; hence a cubic foot of hepatic air weighs, in theſe circumſtances, 574,7089 grains, and 100 cubic inches of it weigh about 33 grains; and its weight, relatively to that of common air, is as 10000 to 9038 *. This hepatic air was extracted from artificial pyrites by marine acid.

The inflammability of this air has been already mentioned. It never detonates with common air; nor can it be fired, in a narrow-mouthed veſſel, unleſs mixed with a conſiderable proportion of this air. M. SCHEELE found it to inflame when mixed with two-thirds of this air. According to M. SENNEBIER, it cannot be fired by the electric ſpark, even when mixed with any proportion of reſpirable air. I found a mixture of one part of hepatic air and 1,5 of common air to burn blue, without flaſhing or detonating. During the combuſtion ſulphur is conſtantly depoſited, and a ſmell of vitriolic air is perceived. A mixture of half hepatic and half nitrous air burns with a bluiſh, green, and yellow lambent flame; ſulphur is alſo depoſited, and in proportion as this is formed, a candle dipped in this air burns more weakly, and is at laſt extinguiſhed. A mixture of two parts nitrous and one of hepatic air partially burns, with a green flame, and a candle is extinguiſhed in the reſiduum, which then reddens on coming in contact with atmoſpheric air. I made a mixture of one part nitrous and one part hepatic air, and to this admitted one part alſo of common air; the inſtant the common air was introduced, ſulphur was precipitated, and the three meaſures occupied the ſpace of 2,4 meaſures; this burned on the ſurface with a wide greeniſh flame, but the candle was extinguiſhed when ſunk deeper.

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* Hence the weight which I have aſſigned to common air in my firſt paper, after M. FONTANA, is evidently erroneous; and, indeed, by that determination common air would not be even 700 times lighter than water, in the temperature of 55°, and the barometer 29,5, which contradicts all barometrical and aeroſtatic experiments: and I cannot omit mentioning the very near agreement of the weight of common air here found with that reſulting from the calculation of Sir GEORGE SHUCKBURGH, it is ſo great as to differ only by 2 grains in a cubic foot.

A mixture of four parts common air and one part hepatic burned blue and rapidly; but a mixture of one part dephlogiſticated and one part hepatic, which had ſtood eight days, went off with a report as loud as that of a piſtol, and ſo inſtantaneouſly that the colour of the flame could ſcarce be diſcerned.

Every ſpecies of hepatic air turns the *tincture of litmus* red. M. BERGMAN ſeems to think, that, if this air were waſhed, it would not produce this effect; yet, when I had paſſed two meaſures of it through one of water, or when I had boiled it out of water impregnated with it, or even when I paſſed that which had already reddened litmus, into a freſh quantity of litmus, it ſtill preſerved the ſame property, which I therefore conſider as eſſential to it.

With reſpect to *ſolubility in water*, hepatic airs extracted from different materials differ conſiderably. In the temperature of 66°, water diſſolves, by ſlight agitation, two-thirds of its bulk of alkaline or calcareous hepatic air, extracted by marine acid; three-fourths of its bulk of martial hepatic air, extracted by the ſame acid; eight-tenths of that extracted by means of the concentrated vitriolic acid, or the dilute nitrous or ſaccharine acids in the temperature of 60°; ſeven-tenths of ſedative hepatic air; nine-tenths of acetous hepatic air, and of that afforded by oil of olives; and its own bulk of that produced from a mixture of ſugar and ſulphur. In general, I imagined that which required moſt heat for its production to be moſt ſoluble: though in ſome inſtances, particularly that of acetous hepatic air, that circumſtance does not take place.

But the moſt remarkable phænomenon attending the union of hepatic air with water is, that it is not permanent. Even water, out of which its own air had been boiled, in a few days after ſaturation with hepatic air grows turbid, and in a few weeks depoſits moſt of it in the form of ſulphur, though the bottle be ever ſo well ſtopped, or ſtand inverted in water or mercury. Yet water no way decompoſes hepatic air by abſorbing it; for the part left unabſorbed by a quantity of water is abſorbable by a larger quantity of water, and burns like other hepatic air. Heat does not expel this air from water, until carried to the degree of ebullition.

No ſpecies of hepatic air, which I have examined, precipitates lime from *lime-water*, except the carbonaceous; and even this ſcarcely produces a ſenſible precipitation, unleſs a large quantity of it be made to paſs through a ſmall quantity of lime-water.

The ſolution of *acetous baro-ſelenite*, (that is, ponderous earth diſſolved in diſtilled vinegar) is rendered brown and turbid by this air, but that of *marine baro-ſelenite* is not altered; nor are the ſolutions of other earths. Metallic ſolutions are affected by it in the ſame manner as by hepatic water, of which I ſhall treat in the fifth ſection.

But of all the teſts of hepatic air, the moſt delicate and ſenſible is the *ſolution of ſilver in the nitrous acid.* This, according as the nitrous acid is more or leſs ſaturated with ſilver, becomes black, brown, or reddiſh brown, by contact with hepatic air however mixed with any other air or ſubſtance. When the acid is not ſaturated, or is in large proportion, the brown or black precipitate, which is nothing but ſulphurated ſilver, is re-diſſolved.

It ſhould alſo be remarked, that all hepatic air is ſomewhat diminiſhed by long ſtanding on mercury, whoſe ſurface is then blackened by it. This is particularly the caſe of carbonaceous hepatic air, which certainly carries over and volatilizes part of the charcoal from which it is extracted, eſpecially that portion of air which comes over in the greateſt heat; this it depoſits on the addition of water.

SECTION III.

Of the Action of Hepatic and other Aerial Fluids on each other.

Six cubic inches of common and ſix of hepatic air being mixed with each other, and ſtanding over mercury for eight days, were not in the leaſt altered in their dimenſions or otherwiſe; though a diminution of a $\frac{1}{120}$th part might be perceived. The mercury was ſlightly blackened. The event was the ſame when three meaſures of common and one of hepatic air were uſed. Water took up the hepatic air. No fixed air was found.

Five meaſures of hepatic, and five of *dephlogiſticated air* ſo pure that one meaſure of it and two of nitrous air made only three-tenths of a meaſure, remained unaltered for eight days, the mercury only being blackened. No fixed air was produced, nor the dephlogiſticated air phlogiſticated. When the mixture was fired, it went off all at once with a loud report.

Four meaſures of phlogiſticated and four of hepatic air remained undiminiſhed for ſixteen days: water then took up the hepatic, and left the phlogiſticated air.

Four meaſures of *inflammable* and four of hepatic air remained unaltered for ſix days.

Two meaſures of hepatic and two of *marine acid* air ſuffered no diminution in three days. The mercury on which they ſtood was not blackened. Water took up both, and precipitated the ſolution of ſilver black.

The ſame quantity of hepatic and *fixed air* remained four days without any ſenſible diminution. Four meaſures of water abſorbed the greater part of both, had an hepatic ſmell, precipitated lime from its ſolution, and alſo ſilver, as uſual. The reſiduum extinguiſhed a candle.

But *vitriolic*, *nitrous*, and *alkaline* airs had very remarkable effects on hepatic air.

Two meaſures of hepatic being introduced to two of *vitriolic air*, a whitiſh yellow depoſition immediately covered both the top and ſides of the jar, and both airs were, without any agitation, reduced to little more than one meaſure; but the opacity of the incruſted glaſs prevented my then aſcertaining the diminution with preciſion. Hence I repeated this experiment more at large, in the following manner. To five meaſures of vitriolic air (each meaſure containing a cubic inch) I added one of hepatic air. In leſs than a minute, without any agitation, the ſides of the glaſs were covered with a whitiſh ſcum, which ſeemed moiſt, and a diminution took place of more than one meaſure. In four hours after, I introduced a ſecond meaſure of hepatic air, which was followed by a ſimilar diminution and depoſit. The next day I added three more meaſures of this laſt, at the interval of four hours between each; and ſtill finding a conſiderable diminution after each, I the following day added another meaſure; the diminution produced by this laſt appeared to me not to exceed one meaſure. I then poured off the reſiduary air into another jar, and found it not to exceed three meaſures; ſo that here eleven meaſures, namely, five of vitriolic and ſix of hepatic air, were reduced to three. Into one meaſure of this reſiduary air I introduced a lighted candle: it was immediately quenched. To the two remaining meaſures I added one meaſure of water: by agitation it took up four-tenths of its bulk. To part of the remainder I added nitrous air, which had no effect upon it. Another part of it extinguiſhed a candle. It had not a vitriolic ſmell.

The water which had taken up four-tenths of its bulk of this air did not precipitate lime; nor did it affect acetous baro-ſelenite in leſs than a quarter of an hour, and then produced a very ſlight cloud. It ſenſibly reddened litmus, and precipitated the ſolution of ſilver white; and hence it appears to have taken up a very minute portion of vitriolic acid. And what was not taken up by water ſeems to have been mere phlogiſticated air.

I afterwards waſhed the ſulphur, which coated the jar, with diſtilled water. This water ſlightly reddened litmus, precipitated not only the acetous, but alſo marine baro-ſelenite copiouſly, as well as marine and nitrous ſelenite; alſo the nitrous ſolutions of ſilver, lead, and mercury, all white. It even precipitated lime from lime-water, forming a cloud in it, which neither the fixed nor volatile acid of vitriol can produce. Hence this water contained nothing hepatic; but, on the contrary, a conſiderable proportion of the aërial and vitriolic acids *.

With *nitrous air* I made the following experiments. Firſt, I found that two meaſures or cubic inches of nitrous and two of hepatic air were little altered when firſt mixed, even by agitation; but after thirty-ſix hours both were reduced to nearly one-third of the whole, but ſomething more. Yellow particles of ſulphur were depoſited both on the mercury, and on the ſides of the jar, but the mercury was not blackened. The reſiduary air had ſtill an hepatic ſmell, and was ſomewhat further diminiſhed by water; and in the unabſorbed part a candle burned naturally. The water had all the properties of hepatic water.

Perceiving by this experiment that I had not employed enough of nitrous air to condenſe the hepatic perfectly, to eight cubic inches of hepatic air I added nine of nitrous air, all at once; a yellowiſh cloud inſtantly appeared, a ſlight white ſcum was depoſited on the ſides of the jar, and the whole ſeemed diminiſhed about two cubic inches, or between one-ninth and one-eighth, the temperature of the room being then 72°. I then laid by the mixture, and in forty-eight hours after, I found the whole reduced to ſix cubic inches, and the top and ſides of the jar covered with a white cake of ſulphur, the heat of the room being conſtantly kept between 60° and 70°. Finding the diminution to reach no further in twenty-four hours more, I examined the reſiduary air. It exhibited the following appearances.

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* Note, the vitriolic acid air here employed was the pureſt poſſible; it was extracted from ſulphur diſtilled with precipitate *per ſe*.

1°, It had the ſmell of alkaline air pretty ſtrongly; at leaſt that ſmell iſſued from the jar that contained it after the air itſelf was poured into another jar.

2°, A candle burned in it naturally.

3°, It did not affect tincture of litmus or lime-water, or acetous barytes.

4°, No ſpecies of air had any effect on it except the dephlogiſticated, with which it produced a ſlight redneſs and diminution.

5°, It produced a ſlight white precipitate in ſolution of ſilver.

It is plain, this air is the ſame as that which Dr. Priestley calls *dephlogiſticated nitrous air*, and which, I think, may more properly be called *deacidified nitrous air*. A further examination of it would lead me too far from the preſent ſubject: I ſhall therefore defer it until another opportunity.

As it appeared to me, from the experiment mentioned in the ſecond ſection (in which I found ſulphur precipitated from a mixture of nitrous and hepatic air, immediately after the admiſſion of common air) that an uncombined acid in the nitrous air was the cauſe of the precipitation of ſulphur; I attempted depriving nitrous air of any looſe acid it might contain, before I ſhould mix it with hepatic air.

1ſt, I made ſome nitrous air from ſilver very carefully over boiled and filtered water, and found it to contain an acid, for it ſtrongly reddened tincture of litmus.

2dly, I admitted alkaline air to this nitrous air until it no longer cauſed any cloud, and then waſhed out the ammoniacal compound in diſtilled water; after which I transferred this purified nitrous air to the mercurial tub. It appeared to loſe, by privation of its acid, about one-ſixth of its bulk; and it was diminiſhed by common air juſt in the ſame manner as unpurified nitrous air is.

Then to 8 cubic inches of this purified nitrous air I admitted all at once 7 cubic inches of hepatic air. No cloud, diminution, or depoſit, appeared; but in ſix hours after (the temperature of the room being all the time at 76°), the whole was reduced to 5 cubic inches; the diminution went no further eighteen hours after. Sulphur, much whiter than in the former experiments, was depoſited, and both in this and in the former experiments that part of it which, by the riſing of the mercury, was intercepted between it and the jar, was of a yellow and red ſhining colour, and not black as that depoſited on mercury uſually is. The reſiduary air flaſhed with ſo much vehemence as to extinguiſh the candle dipped into it, by the violence of the blaſt. The flame was exceeding white and vivid; but it did not detonate in the leaſt, but rather reſembled dephlogiſticated air. The jar out of which it had been transferred had a ſharp alkaline ſmell.

This air was not in the leaſt diminiſhed by nitrous air, even when heated to 150 degrees; which heat I contrived to produce by paſſing the upper part of the jar that contained this air into another wider jar, furniſhed with a perforated cork bottom, and filling this with water heated to that degree.

Water poured into the jar in which the ſulphur was depoſited, produced a bluiſh white cloud in ſolution of ſilver, though inſipid to the taſte.

Hence it appears to me, that, whatever this air may be, it had been deacidified by hepatic air ſtill more perfectly than that in which a candle burns naturally; and that it is by no means dephlogiſticated.

Laſtly, *Alkaline* and hepatic airs, perfectly pure and mixed in proper proportions, would probably deſtroy each other completely, though I have not been able to effect this intirely. Six meaſures of hepatic air from liver of ſulphur and 6 of alkaline air immediately throw up a white cloud, leave a whitiſh ſcum on the ſides of the jar, and are reduced to about 1 meaſure. On adding water this is reduced to about one-half; and in this I found a candle to burn naturally: but the following experiments, being made with more care, prove that this reſiduary air was only the common air of the veſſels.

To 6 cubic inches of calcareous hepatic air I admitted, all at once, 7 of alkaline air; a white cloud and a little white ſcum at firſt appeared; but in a few ſeconds the whole was reduced to ſix-ſevenths of a cubic inch; and on adding 2 meaſures of water, only one-ninth of a cubic inch of air remained. This could not be inflamed. The water, thus impregnated, precipitated a ſolution of ſilver black. In this experiment great care was taken to have each of the mixed airs as pure as poſſible, and the alkaline was admitted all at once, inſtead of by different portions, merely with that view; and it is probable, that, if the due proportion were hit upon, nothing would remain. The ſcum appears to be almoſt liquid, and as ſoon as the jar is emptied of mercury, it breaks out into a white ſmoke, with an exceeding ſharp urinous ſmell.

Five meaſures of martial hepatic air were, upon the admiſſion of 5½ of alkaline air, reduced to ſomething more than one meaſure, and upon the addition of water there remained but half a meaſure; and this was inflammable, with detonation; the inflammable air undoubtedly proceeding from the ſolution of the iron.

Five cubic inches of ſaccharine hepatic air, mixed with 5 of alkaline air, were diminiſhed more ſlowly; for after five minutes there ſtill remained 4,5 cubic inches. I then added another meaſure of alkaline air: in three hours after there remained but 1,25 cubic inches. In paſſing this reſiduum through water it was reduced to about half a cubic inch; and this burned with a blue lambent flame, without leaving a vitriolic ſmell or any depoſit on the glaſs; ſo that it clearly was inflammable air from the ſugar.

I once imagined I had obtained inflammable air from a mixture of alkaline air with hepatic air drawn from liver of ſulphur; but I afterwards found this inflammable air proceeded from a very ſlight contamination of zinc in the mercury over which my airs had been produced; the alkaline air acted on this zinc, and muſt have produced the inflammable air; for when I afterwards received and mixed theſe airs over mercury, perfectly purified, I obtained no more inflammable air.

SECTION IV.

Of the Action of Hepatic Air, and Acid, Alkaline, and Inflammable Liquids, on each other.

One meaſure of *oil of vitriol*, whoſe ſpecific gravity was 1,863, abſorbed two meaſures of hepatic air all to one-tenth. The acid was whitened by a copious depoſition of ſulphur. I alſo introduced, over mercury, a meaſure of red *nitrous acid*, whoſe ſpecific gravity was 1,430, to an equal meaſure of hepatic air; red vapours inſtantly aroſe, and only one-tenth or one-twelfth of a meaſure remained in an aërial form; but as the acid acted on the mercury, I was obliged to carry the jar into the water tub, by which means the whole was abſorbed: no ſulphur was here precipitated.

I repeated this experiment in another manner. Having produced 4,5 meaſures of hepatic air over mercury, I transferred them to the water tub, and inſtantly by means of a ſyphon blew into them one meaſure of the above concentrated nitrous acid; but though I managed as quickly as poſſible, the hepatic air was ſomething diminiſhed by contact with the water, before the acid had entered the tube that contained the air. I then ſtopped the tube with a ground glaſs ſtopper, and laid it by for twelve hours; after which interval I found the liquor in the tube white and turbid, and but weakly acid, much water having entered in ſpite of my endeavours to exclude it. The remaining air ſlightly detonated on preſenting to it a lighted candle, and had an hepatic ſmell. But as this hepatic air was obtained from ſulphureo-martial paſte, it does not prove that inflammable air enters into the compoſition of other hepatic airs, derived from the union of ſulphur with ſubſtances that do not yield inflammable air.

Finding it ſo difficult to ſubject hepatic air to the direct action of the concentrated nitrous acid, I diluted it to that preciſe degree at which it could not act on mercury without the aſſiſtance of heat, and then paſſed through it an equal bulk of the ſame hepatic air; the acid was whitened, and eight-tenths of the air abſorbed, and the reſiduum detonated. Repeating the ſame experiment with hepatic air from liver of ſulphur, I found ſtill more of it abſorbed by the acid; but the reſiduum no longer detonated, but burned with a blue and greeniſh flame, and ſulphur was depoſited on the ſides of the jar.

Obſerving this dilute acid to abſorb nearly three times its bulk of alkaline hepatic air, I expelled this air from it by heat, but obtained only one-ſixth of the air that had been abſorbed; and in this a candle burned naturally.

Two meaſures of alkaline hepatic air, being expoſed to one of ſtrong *marine acid*, were abſorbed, by ſlight agitation, all to one-fifth of a meaſure. A third meaſure of air being then added, there remained, after ſome agitation, but half a meaſure. Sulphur was precipitated as uſual; but the mercury over which the acid ſtood attracted it from the acid; for it was blackened, which did not happen when the former acids were uſed. The reſiduum burned juſt as pure hepatic air.

Diſtilled vinegar abſorbs nearly its own bulk of air, and becomes ſlightly whitened; but by agitation it may be made to take up about twice it bulk, and then becomes very turbid.

One meaſure of *cauſtic vegetable alkali*, whoſe ſpecific gravity was 1,043, abſorbed nearly four meaſures of alkaline hepatic air. It was at firſt rendered brown by it; but after ſome time it grew clear, ſulphur was depoſited, and the ſurface of the mercury blackened. This ſhews that alkalies are not dephlogiſticated by ſilver or other metals, as Mr. BAUMÉ imagined, but only cleared of part of the ſulphur, which they commonly contain, it being formed by the tartar vitriolate contained in the plant, and coal, during combuſtion.

One meaſure of *cauſtic volatile alkali*, whoſe ſpecific gravity was 0,9387, abſorbed 18 of hepatic air. If the cauſtic liquor contained more alkali, it would abſorb more hepatic air, as 6 meaſures of hepatic unite to 7 of alkaline air; and thus the ſtrength of alkaline liquors, and their real contents, may be determined better than by any other method. Alſo the ſmoaking liquor of BOYLE, which is difficultly prepared in the uſual way, may eaſily be formed by placing the volatile alkali in the middle glaſs of Dr. NOOTH's apparatus for making artificial mineral waters, and decompoſing artificial pyrites, or liver of ſulphur, in the lower glaſs, by marine acid.

Oil of olives abſorbs nearly its own bulk of this air, and obtains a greeniſh tinge from it.

But *new milk* ſcarcely abſorbs one-tenth of its bulk of this air, which is very remarkable, and is not in the leaſt coagulated.

Oil of turpentine alſo abſorbs its own bulk of this air, and even more; but then becomes turbid. Water ſeems alſo to precipitate this air from it, for when ſhaken with it a white cloud appears.

Spirit of wine, whoſe ſpecific gravity was 0,835, abſorbed nearly three times its bulk of this air, and became brown. By this means ſulphur may be combined with ſpirit of wine much more eaſily than by Count LAURAGAIS' method, the only hitherto known. Water precipitates the ſulphur in part.

Sulphurated ſpirit of wine does not tinge litmus red; but it precipitates lime-water, as highly rectified ſpirit of wine ſingly does. It alſo precipitates and gives a brown colour to acetous baro-ſelenite, which ſpirit of wine alone alſo does. It turns the ſolution of ſilver black and reddiſh brown. Concentrated vitriolic acid precipitates the ſulphur from it, which neither the nitrous nor marine acids can effect.

When hepatic air is mixed with an equal bulk of *vitriolic æther*, the bulk of the air is at firſt increaſed; but afterwards half of it is abſorbed, and a ſlight precipitation appears. The ſmell of the æther is mixed with that of the hepatic air; but on adding water it becomes very offenſive, reſembling that of putrefying animal ſubſtances.

To one meaſure of hepatic air I added 1,5 of the *nitrous ſolution of ſilver:* the air was immediately, without agitation, reduced to half a meaſure, and the ſolution blackened. The remaining air admitted a candle to burn naturally. Hepatic air was alſo abſorbed, but not ſo readily, nor in ſuch quantity, by the ſolution of *vitriols of iron and ſilver*; that of ſilver was blackened; that of iron at firſt became white, but by agitation darker. The reſiduary air burned blue, as hepatic air uſually does.

SECTION V.

Of the Properties of Water ſaturated with Hepatic Air.

This water turns tincture of litmus red.

It does not affect lime-water.

It does not form a cloud in the ſolution of marine, though it does in that of acetous baro-ſelenite.

The ſolutions of other earths in the mineral acids are not altered by it.

When dropped into a ſolution of *vitriol of iron* or marine ſalt of *iron*, it produces a white precipitate.

In nitrous ſalt of *copper* it cauſes a brown precipitate, and the liquor is changed from blue to green. The precipitate rediſſolves by agitation. In vitriol of copper it forms a black precipitate.

The ſolution of *tin* in aqua regia is precipitated by it of a yellowiſh white colour; that of *gold*, black; that of *regulus of antimony*,

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antimony, red and yellow; that of *platina*, red mixed with white.

The ſolution of *ſilver* in the nitrous acid, and alſo that of *lead*, whether in the acetous or nitrous acid, are precipitated black. If the ſolutions are not perfectly ſaturated with metal, the precipitates will be brown or reddiſh brown, and may be rediſſolved by agitation.

The nitrous ſolution of *mercury* is precipitated of a yellowiſh brown; that of ſublimate corroſive, yellow mixed with black; but by agitation it becomes white.

The nitrous ſolution of *biſmuth* becomes, by mixture with this water, reddiſh brown, and even aſſumes a metallic appearance; that of *cobalt* becomes dark; that of *zinc*, of a dirty white; that of *arſenic*, in the ſame acid, yellow mixed with red and white, orpiment and realgar being formed.

If *oil of vitriol*, whoſe ſpecific gravity is 1,863, be dropped into hepatiſed water, it renders it ſlightly turbid; but, if the *volatile vitriolic acid* be dropped into it, a bluiſh white and much denſer cloud is formed in the water.

Strong nitrous acid, whether phlogiſticated or not, cauſes a copious white precipitation; but *dilute* nitrous acid produces no change. *Green* nitrous acid, whoſe ſpecific gravity was 1,328, immediately precipitated ſulphur from it.

Strong marine acid produced a ſlight cloud; but neither diſtilled vinegar nor acid of ſugar had any effect.

It is ſaid by Mr. BERGMAN, that hepatiſed water in a well cloſed veſſel effects a ſolution of iron in a few days; but this experiment, on repeated trials, did not ſucceed with me: nor could I diſſolve any other metal in this water; the ſulphur indeed unites to many of them, but forms an inſoluble maſs;

ſo that, I preſume, metallic ſubſtances can never be found in hepatiſed mineral waters.

SECTION VI.

Of the Properties of Alkaline Liquors impregnated with Hepatic Air.

I have already mentioned the proportion of air they are able to take up. Colourleſs fixed alkaline liquors receive a browniſh tinge from this air. The reſiduum they leave is of the ſame nature as the part they abſorb.

A cauſtic fixed alkaline liquor, ſaturated with this air, precipitates *barytes* from the acetous acid, of a yellowiſh white colour. It alſo decompoſes other earthy ſolutions, and the colour of the precipitates varies according to their purity, and perhaps this teſt might be ſo far improved as to ſupply the place of the Pruſſian alkali.

It precipitates the ſolution of vitriol of *iron*, and alſo marine ſalt of iron, black; but this latter generally whitens by agitation. That which I uſed was very ſaturate.

The ſolutions of *ſilver* and *lead* are alſo precipitated black with ſome mixture of white; that of *gold* is alſo blackened; but that of *platina* becomes brown.

Solutions of *copper* let fall a reddiſh black or brown precipitate.

Sublimate corroſive by this teſt diſcovers a precipitate partly white and black, and partly orange and greeniſh.

In the nitrous ſolution of *arſenic* it forms a yellow and orange; and in that of *regulus of antimony*, in aqua regia, an orange precipitate mixed with black.

Nitrous ſolution of *zinc*, thus treated, ſhews a dirty white; that of *biſmuth* a brown mixed with white; and that of *cobalt* a brown and black precipitate.

As *Pruſſian alkali* always contains ſome iron, it gives a purple precipitate with this teſt, which precipitate is eaſily diſſolved.

It turns tincture of raddiſhes, which is my teſt for alkalies, green.

The action of liver of ſulphur on metallic ſubſtances in the *dry way* is deſcribed by many authors, and particularly in an excellent Diſſertation by M. ENGESTROM; but its action in the *moiſt* way has not been mentioned, as far as I recollect, by any. Hence I tried its effect on a few grains of iron, copper, lead, tin, zinc, biſmuth, regulus of antimony, and of arſenic. Putting each into a bottle, containing about three half ounces of liquid liver of ſulphur, ſo far diluted that its colour was yellow; in about fifteen days I found they all, except the zinc and tin, had attracted ſulphur from the fixed alkali. Iron, arſenic, and regulus of antimony and lead, were moſt altered; copper next, and biſmuth leaſt: but the liquors held none of the metals in ſolution; that which contained iron became green; on adding an acid ſulphur was precipitated; if it held iron it could not at that period be detected.

Water ſaturated with the condenſed reſiduum of alkaline and hepatic air, that is, with the pureſt volatile liver of ſulphur, does not precipitate marine *ſelenite*, though it forms a ſlight brown and white cloud in that of marine *baro-ſelenite*.

It produces a black precipitate in the ſolution of vitriol of *iron*, and a black and white in that of marine ſalt of iron; but by agitation this laſt becomes wholly white.

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It precipitates both vitriol of copper, and the nitrous ſalt of copper, red and brown.

Tin in aqua regia gives a yellowiſh precipitate; *gold* a dilute yellow and reddiſh brown; *platina* a fleſh-coloured precipitate; and *regulus of antimony* a yellowiſh red.

Silver is precipitated black; and ſo is *lead* both from the nitrous and acetous acids.

Sublimate corroſive appeared for an inſtant red; but ſoon after its precipitate appeared partly black and partly white.

The nitrous ſolution of *biſmuth* affords alſo a precipitate, partly black, partly white, and partly reddiſh brown, and of a metallic appearance; that of *cobalt* is alſo black or deep brown.

Arſenical ſolutions give yellow precipitates more or leſs red; but thoſe of *zinc* only a dirty white.

All theſe colours vary in ſome degree, according as the liquors are more or leſs ſaturated previous to and after their mixture, and the time they have ſtood together.

SECTION VII.

Of the Conſtitution of Hepatic Air.

From an attentive conſideration of the above experiments, which I purpoſely diſengaged from all theory, it is difficult to conclude, that hepatic air conſiſts of any thing elſe than ſulphur itſelf, kept in an aërial ſtate by the matter of heat. Every attempt to extract inflammable air from hepatic air, when drawn from materials that previouſly contained nothing inflammable, namely, from alkaline or calcareous hepars,

proved

proved abortive: on the contrary, when the materials could previouſly ſupply inflammable air, as when martial carbonaceous and ſaccharine compounds were employed, inflammable air, in ever ſo ſmall a proportion, was detected: nor could hepatic air be procured from the direct union of inflammable air and ſulphur, as we have ſeen.

Some have imagined, that this air conſiſts of liver of ſulphur itſelf volatilized, and conſequently that an alkali enters into its compoſition; but many weighty reaſons oppoſe this ſuppoſition. In the firſt place this air is evidently, though weakly acid, ſince it reddens litmus, and precipitates acetous baro-ſelenite. 2dly, It may be extracted from materials that either contain no alkali at all, or next to none, as iron, ſugar, oil, charcoal: and, laſtly, it is not decompoſed by marine or fixed air, by which, nevertheleſs, liver of ſulphur is decompoſable.

I formerly thought that ſulphur was held in ſolution in hepatic air, either by vitriolic or marine air; yet though both of them may hold ſulphur in ſolution, as we have ſeen, ſtill neither of them is eſſential to the conſtitution of hepatic air as ſuch, ſince it is producible from materials that contain neither of theſe acids; and, from whatever ſubject it is obtained, it exhibits the characters of one and the ſame acid, namely, the *vitriolic exceedingly weakened*; and ſuch an acid we may ſuppoſe ſulphur itſelf to be.

In effect, ſulphur, even in its concrete ſtate, affords many characters of acidity. It unites with alkalies, calcareous and ponderous earths, and moſt metals, as a weak acid might: and except a manifeſt ſolubility in water (a property which ſome other concrete acids alſo poſſeſs in a very weak degree) it exhibits every character of acidity. But its acidity is the weakeſt poſſible, ſince it decompoſes only acetous, and not marine baro-ſelenite, and is ſeparable from alkalies and earths by all other acids.

That the matter of heat enters into the compoſition of this air, is evident from the experiments of M. Scheele, who paid particular attention to that object. He found, that acids excite much leſs ſenſible heat in uniting with *liver of ſulphur*, whether alkaline or calcareous, than while uniting with a proportion of cauſtic fixed alkali or lime equal to that which enters into the compoſition of thoſe *livers*; whence he juſtly infers, that the *difference* enters into the compoſition of the hepatic air produced. I have proved the ſame thing another way: for, inſtead of decompoſing an *alkaline* hepar by marine acid, I tried to decompoſe it by a ſaturate ſolution both of marine ſelenite and marine Epſom. The decompoſition indeed took place, but no hepatic air was produced: for the acid having given out its ſpecific heat on uniting to the earths, had none to loſe or communicate on uniting to the alkali, and conſequently the ſulphur receiving none could not be thrown into an aërial ſtate.

It is remarkable, that bodies capable of an aërial form receive the latent heat neceſſary for that form, much more readily from a body that parts with its ſpecific heat than by the mere application of ſenſible heat. Thus aërated barytes cannot be decompoſed by mere heat, as Dr. Withering has ſhewn, though its air is eaſily ſeparated from it by an acid; and in the ſame manner antimony cannot be deſulphurated even by vitrefaction, though it may by acids: ſo liver of ſulphur will not give hepatic air by mere heat, though it will by the intervention of an acid, even the weakeſt. The reaſon of which ſeems to be this: the matter of heat has no particular affinity with any ſubſtance, as is evident from its paſſing indifferently from any hot body to a colder, of whatever ſort or kind the bodies may be; but it is determined to unite with this or that body in a latent ſtate, in greater or leſſer quantity, in proportion to the greater or leſſer *capacity* of theſe bodies to receive it. Now acids, by uniting to the alkaline baſis of liver of ſulphur, expel the ſulphur, and give it their heat, at the *inſtant* the ſulphur, by its ſeparation, has the capacity to receive it; whereas ſenſible external heat, acting alike on both the conſtituent parts of liver of ſulphur, ſeparates neither; or if it ſeparates them, yet, by its *ſucceſſive* action, it throws one of them into a *vaporous* ſtate firſt, and bodies that *firſt* acquire this ſtate can never after acquire an aërial ſtate by any *ſubſequent* acceſſion of heat.

The vitriolic and nitrous acids are leſs adapted to the production of hepatic air than the marine acid, though they contain more ſpecific heat than the mere acid part of the marine acid: the moſt probable reaſon of which is, becauſe they have a ſtronger attraction to ſulphur itſelf, and ſo detain it.

Hepatic air is much diſpoſed to give out its latent heat, particularly when in contact with ſubſtances to which it has any affinity; thus it is condenſed in water in a few days; it is alſo condenſed by long expoſure to freſh ſurfaces of mercury or ſilver or other metals, particularly if they are moiſt. M. BERGMAN found it in great meaſure condenſed into ſulphur, when incloſed alone in a bottle *. In this caſe it probably contained an exceſs of ſulphur; for hot hepatic air is capable of keeping a farther quantity of ſulphur in ſolution, and depoſing it when cold, as I have frequently obſerved.

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* See a note in the ſecond volume of M. MORVEAU's tranſlation of the ſecond volume of BERGMAN's Works, p. 341.

The precipitation of metallic ſubſtances by this air is owing partly to the union and phlogiſtication of the acids by this air, and partly to its union with the metals themſelves, for it evidently unites in moſt caſes to both.

As alkalies and ſulphur are known to have an affinity to each other, we eaſily underſtand why hepatic and alkaline airs are condenſed when mixed with each other; nor is there any difficulty in conceiving why hepatic air is not condenſed by common, dephlogiſticated, inflammable, or phlogiſticated airs, or remarkably by marine air; but it ſeems very extraordinary, that hepatic air and vitriolic air ſhould be condenſed, and in great meaſure converted into ſulphur by their mutual action on each other, particularly as they both ſeem nearly of the ſame ſpecies, or at leaſt nearly allied to each other. The attraction of two bodies, thus circumſtanced, is certainly very extraordinary; yet that their union proceeds from attraction ſeems pretty evident, for concentrated vitriolic acid, and particularly volatile vitriolic acid, precipitates ſulphur copiouſly from hepatiſed water. Volatile vitriolic acid frequently holds ſome ſulphur in ſolution, as appears from the experiments of Dr. PRIESTLEY and M. BERTHOLLET; and hence it depoſits ſome when it loſes its aërial form, or by mere length of time; but the whole of this condenſed air is not turned into ſulphur, for the water that waſhed the precipitated ſulphur took up a quantity of volatile acid and fixed air, as has been ſhewn.

The condenſation of hepatic air by nitrous air ſeems owing to the ſame cauſe; for when the nitrous air was in great degree deprived of ſuperfluous acid, the condenſation of the hepatic was much ſlower; and that which after all took place ſeems to have been effected by the decompoſition of the nitrous air, and the conſequent extrication of an acid.

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The decompoſitions effected by fixed and volatile livers of ſulphur obviouſly proceed in moſt caſes from a double affinity.

SECTION VIII.

Of Phoſphoric Hepatic Air.

As phoſphorus, in reſpect to its conſtituent parts, bears a ſtrong reſemblance to ſulphur, I was naturally led to examine its phænomena when placed in the ſame circumſtances: I therefore gently heated about 10 or 12 grains of phoſphorus, mixed with about half an ounce of cauſtic fixed alkaline ſolution, in a very ſmall phial, furniſhed with a bent tube, and received the air over mercury. Upon the firſt application of heat two ſmall exploſions took place, attended with a yellow flame and white ſmoke, which penetrated through the mercury into the receiver; theſe were followed by an equable production of air. At laſt the phoſphorus began to ſwell and froth, and fearing the rupture of the phial, I ſtopped the tube to prevent the acceſs of atmoſpheric air, and removed the phial to a water tub, intending to throw it in; but in the mean while the phial burſt with a loud exploſion, by reaſon of an obſtruction in the tube, and a fierce flame immediately iſſued from it. However I obtained about 8 cubic inches of air.

This air was diminiſhed very ſlightly, by agitation with an equal bulk of water, and then became cloudy like white ſmoke, but ſoon after recovered its tranſparency. Upon turning up the mouth of the tube to examine the water, the unabſorbed air inſtantly took fire, and burned with a yellow flame without exploding, leaving a reddiſh depoſit on the ſides of the tube.

Water impregnated with phoſphoric air, and over which this air had burned, ſlightly reddened tincture of litmus:

Did not affect Pruſſian alkali:

Had no effect on the nitrous ſolutions of *copper* or *lead*, *zinc* or *cobalt*, nor on marine ſolution of *iron* or *tin*, or of tin in aqua regia, nor on the vitriolic ſolutions of *iron*, *copper*, *tin*, *lead*, *zinc*, *regulus of antimony*, *arſenic*, or *mangane*ſ*e*; nor on the marine ſolutions of *iron*, *copper*, *lead*, *zinc*, *cobalt*, *arſenic*, or *mangane*ſ*e*.

But it precipitated the nitrous ſolution of *ſilver* black, and vitriol of ſilver brown; alſo nitrous ſolution of *mercury* made without heat brown and black; but vitriol of mercury firſt became reddiſh, and afterwards white; and ſublimate corroſive yellow and red mixed with white.

Gold diſſolved in aqua regia is precipitated purpliſh black, and from the vitriolic acid browniſh red and black; but *regulus of antimony* in aqua regia is precipitated white by this phoſphorated water.

The nitrous ſolution of *biſmuth* ſhewed firſt a white, and preſently after a brown precipitate. Vitriol of biſmuth and marine ſalt of biſmuth were alſo precipitated brown; this latter re-diſſolved by agitation.

The nitrous ſolution of *arſenic* alſo became brown, but re-diſſolved by agitation.

I afterwards impregnated ſome water with this air, without ſuffering the air to burn over it: it ſcarcely affected litmus, did not precipitate lime-water; but it cauſed a black precipitate in ſolution of *ſilver*, a white precipitate of *regulus of antimony* in AR, and a whitiſh yellow in that of *ſublimate corroſive*.

To a meaſure of this air I let up a meaſure of water, and through this ſome ſmall bubbles of *common air*; every bubble flamed and produced a white ſmoke until about half as much common air was introduced as there was originally of phoſphoric; and yet the original bulk did not appear increaſed; the flame each time produced a ſmall commotion, and a ſmoke deſcended after inflammation into the water: when flame ceaſed to be produced, ſmoke ſtill followed the introduction of more common air. Bubbles of phoſphoric air, eſcaping through mercury into the atmoſphere, flame, crackle, and ſmell, exactly like the electric ſpark *.

To a meaſure of phoſphoric air I let up a half meaſure of *nitrous air*: a white ſmoke appeared, with an exceeding ſlight diminution, and the tranſparency was ſoon reſtored, a ſlight ſcum being depoſited on the ſides of the jar. Another half meaſure of nitrous air produced no ſmoke or diminution; but on adding water, and agitating the air in it, much more of it was abſorbed. Upon turning up the jar the nitrous air firſt eſcaped in the form of a red vapour, and this was followed by a whitiſh ſmoke. The water had a phoſphoric ſmell, and precipitated the ſolution of ſilver brown. In this experiment the acid of the nitrous air ſeems to have acted the ſame part that it does in hepatic air.

Phoſphoric air was ſcarce at all diminiſhed by the addition of an equal meaſure of *alkaline air*; and water being put up to theſe, took up in appearance little elſe than alkaline air, yet on turning up the mouth of the jar, the reſiduary air ſmoked without flaming.

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* A few months after I made theſe experiments on phoſphoric air, the tenth volume of the Mémoires des Savans Etrangers was publiſhed; and in this I found, that the ſpontaneous inflammation of this air was known to M. GINGEMBRE in the year 1783. His experiments are now publiſhed in ROZIER's Journal for October, 1785.

The water, thus impregnated, had exactly the ſmell of onions.

It turned tincture of radiſhes green.

It precipitated ſolution of *ſilver* black; and that of *copper* in the nitrous acid brown; but this precipitate was re-diſſolved by agitation, and the liquor became green. *Sublimate corroſive* was precipitated yellow mixed with black.

Iron was precipitated white, both from the vitriolic and marine acids; but a pale yellow ſolution of it in the nitrous acid was not affected; and a red ſolution of it in the ſame acid was only congrumated.

Regulus of antimony in aqua regia gave a white, *cobalt* in nitrous acid a very ſlight reddiſh, and *biſmuth* in the ſame acid a brown precipitate.

But neither the nitrous ſolution of *lead* or *zinc*, nor that of *tin* in marine acid or aqua regia, nor that of *regulus of antimony* in aqua regia, were any way affected.

Fixed air, mixed with an equal proportion of phoſphoric air, produced a white ſmoke, ſome diminution, and a yellow depoſit. On agitating the mixture in water, the fixed air was taken up all to one-tenth. The reſiduum ſmoked, but did not inflame ſpontaneouſly.

To a ſmall portion of phoſphoric air I introduced ſome *precipitate per ſe*. It ſoon grew black, and a white ſmoke appeared. In two days the precipitate remained ſolid, yet acquired a ſhining pale white colour, like that of ſteel: the air loſt its ſpontaneous inflammability; but I am not certain that this want of inflammability did not proceed from ſome other cauſe; for two days after I made this air, I found a quantity of it, which had reſted all night on water, had depoſited a yellow ſcum on the ſides of the jar, and loſt its ſpontaneous inflammability next morning. The temperature of the air was then 53°; and when it inflamed before, the temperature was 68°.

From theſe few experiments, which the ſmall quantity of air I then obtained did not ſuffer me to repeat, I think we may conclude, that phoſphoric air is nothing elſe but phoſphorus itſelf in an aërial ſtate, and differs from ſulphur in this, among other points, that it requires much leſs latent heat to throw it into an aërial form, and hence may be diſengaged from fixed alkalies, without the aſſiſtance of an acid.

VI. *Obſervations on the Affinities of Subſtances in Spirit of Wine. In a Letter to* Richard Kirwan, *Eſq. F. R. S. by* John Elliot, *M. D.*

Read January 19, 1786.

SIR,

IN your excellent papers on the attractive powers of the mineral acids, you ſhew that metallic calces have ſtronger attractions to thoſe acids, than alkalies and earths. The following experiments not only confirm this doctrine, but alſo a poſition that I have lately ventured to advance*, "that certain decompoſitions will take place in ſpirit of wine, which "will not at all in water, nor in the *dry* way."

I have ſhewn, that if expreſſed oil be mixed with ſlaked lime into a paſte, ſo as to form calcareous ſoap, and mild alkali be added, the latter will not decompoſe the former, either in water or by fuſion. But that if ſpirit of wine be ſubſtituted for water, an alkaline ſoap and mild calcareous earth will be formed. As ſea ſalt contains the foſſil alkali, and as by your table of affinities acids have ſtronger attraction to metallic calces than to alkalies, I concluded, that if ſea ſalt were added to a metallic ſoap, a ſimilar double decompoſition would take place.

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* In an Appendix to the ſecond edition of the "Elements of the Branches of "Natural Philoſophy connected with Medicine."

To try this I took ſome diachylum, which had been bought at Apothecaries-Hall, and added to it ſea ſalt; then covered them to a ſufficient height with ſpirit of wine, and ſet the bottle over the fire. Soon after they had boiled, the decompoſition of the diachylum began to be apparent. When the boiling had continued ſome time, I removed the veſſel from the fire, and after it had ſtood a few minutes, decanted the clear liquor while hot; then evaporating it, obtained a true alkaline ſoap. The reſiduum of courſe contained a quantity of calx of lead, combined with marine acid.

But much of the diachylum remained either wholly or partly undecompoſed: I therefore added more ſea ſalt and ſpirit of wine, and obtained a further yield of ſoap. But though much ſea ſalt remained behind, diachylum was ſtill found in the reſiduum. I found, indeed, that if the ingredients were previouſly freed from their water, the proceſs ſucceeded to ſomewhat better advantage.

From five ounces of diachylum I did not get quite three ounces of ſoap. This ſoap was likewiſe ſoft, and contained a portion of oil not combined with a ſufficient quantity of alkali. The oil, I ſuppoſe, had exiſted in a ſimilar ſtate in the diachylum: and I remarked, that as the ſpirit evaporated, it gave out the true ſoap firſt, the unſaturated oil not till afterwards; ſo that the latter might eaſily be obtained ſeparate from the former.

If too much ſalt was employed, much of it was taken up by the liquid, and communicated to the ſoap, at leaſt if the ingredients had not been previouſly deprived of their water. To ſeparate this ſalt I diſſolved the ſoap in hot water. When the liquor was cold, the ſoap floated at top, the ſalt remaining in the water underneath. If too little ſalt was uſed, this inconvenience

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inconvenience did not happen, or not in ſo great a degree, though then leſs ſoap was of courſe obtained.

As diachylum, though with a greater proportion of litharge, and boiled longer than that I had from the Hall, ſtill contained oil not ſufficiently ſaturated, I made the metallic ſoap in another way. To a ſolution of ſugar of lead in water I added a ſolution of alkaline ſoap in the ſame liquid. A double decompoſition took place, the oil uniting with the calx of lead, the alkali with the acid of ſalt. Uſing this metallic ſoap inſtead of the other, I obtained an alkaline ſoap harder and more perfect than in the preceding proceſs; but ſtill found that part of the oil remained with the calx of lead in the reſiduum, and adhered ſo firmly, that repeated quantities of ſea ſalt and ſpirit of wine did not wholly ſeparate it.

As I have given this proceſs more with a philoſophical view than any other, I have been thus particular in my account of it, to ſhew that however eligible it may appear at firſt view, it will not anſwer for making ſoap for common ſale. The alkali indeed is procured much cheaper than from barilla, as the lead may be revived and re-calcined into litharge. But the whole of the oil or fat cannot eaſily be converted into ſoap, though in order the better to effect it, I have mixed ſand with the diachylum: and as the oil and litharge muſt, in the large way, be united by boiling, a conſiderable part of the former will not be ſufficiently ſaturated. Fuel muſt be uſed, not only for forming the metallic ſoap, but likewiſe for decompoſing that ſoap, and then diſtilling off the ſpirit, which will alſo require additional time and labour. The quantity of ſpirit of wine loſt, though the proceſs (ſo far as that liquid is concerned) be performed in a ſtill, will alone nearly counterbalance the ſaving in reſpect to alkali. And in the proceſs itſelf there

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there is conſiderable danger, not only of the ſpirit taking fire from the careleſſneſs of the workmen, but likewiſe from the frequent exploſions that happen during the decompoſition of the metallic ſoap.

As in the experiment with calcareous earth and mild alkali, ſo in this, I found that the decompoſition would not take place when water was uſed, nor by fuſion. In the latter caſe, I found that the ſalt was ſo ſtrongly attracted, that it quitted its water of cryſtalliſation to unite to the metallic ſoap. If ſpirit of wine was added to this maſs, a double decompoſition took place, as already deſcribed.

Inſtead of ſea ſalt, I added to diachylum GLAUBER's ſalt, freed from its water of cryſtalliſation by heat. I expected that it would have acted on the metallic ſoap more ſpeedily than the ſea ſalt; but the contrary appeared on trial. On adding a ſmall quantity of ſal ſodæ, the decompoſition went on better, and ſufficiently to ſhew that the ingredients were capable of acting on each other. And I ſuppoſe, from your table, that other neutral and earthy ſalts will have a ſimilar effect, eſpecially if deprived of any ſuperfluous acid by the addition of a little alkali or earth; though I have not made the trials.

Profeſſor BERGMAN has divided his table into two parts; the affinities as they take place in the *moiſt*, and in the *dry* way. But theſe experiments ſhew, that in the *moiſt* way the affinities take place differently, according as water, or ſpirit of wine, is uſed. Perhaps a like difference would be found on uſing other liquids, each of which would probably afford a different table: for much depends on the attraction which the ingredients themſelves have to the liquid employed, as I have endeavoured to ſhew in the work before referred to; for the liquid is to be conſidered as one of the ingredients.

I beg leave to add, Sir, that I think I have ſince hit upon a better method of making ſoap, and without ſpirit of wine; but as I have not yet made all the experiments on this ſubject that I intended, I cannot at preſent give you an account of them. If they ſucceed, I will take the liberty to acquaint you with the reſult.

I am, Sir, with the greateſt reſpect, &c.

J. ELLIOT.

Great Marlborough-Street,
October 31, 1785.

P. S. Since writing the above I have found, that if mild fixed alkali be added to diachylum in hot water, they unite into a gelatinous maſs, which is miſcible with the water. This may be conſidered as a kind of *hepar*. If this ſubſtance be put into hot ſpirit of wine, the decompoſition already deſcribed takes place. If chalk be ſubſtituted for alkali, there is a ſimilar reſult. I have found that nitre is decompoſed by diachylum in ſpirit of wine. I have alſo found, that if the compound of diachylum and common ſalt be put into hot ſpirit of turpentine, the diachylum is diſſolved, but the ſalt remains at the bottom of the veſſel.

* * *

VII. *An Account of ſome minute* Britiſh *Shells, either not duly obſerved, or totally unnoticed by Authors. In a Letter to Sir* Joſeph Banks, *Bart. P. R. S. by the Rev.* John Lightfoot, *M. A. F. R. S.*

Read January 26, 1786.

DEAR SIR,

AS you were pleaſed to think a few ſhells, which I lately ſubmitted to your inſpection, might not be unworthy the notice of the Royal Society; encouraged by ſo reſpectable an opinion, I ſhall now beg leave to lay before you ſome Drawings which I have cauſed to be made of them, together with ſuch remarks concerning them as may tend, in ſome degree, to illuſtrate their natural hiſtory.

The firſt I ſhall mention is an univalve, coiled up into a ſpiral form, the cavity of which is divided into three, four, or more diſtinct chambers or apartments by ſolid tranſverſe *ſepta*, which communicate with each other by a *triradiated* aperture.

Theſe characters accord with no genus of ſhells, hitherto eſtabliſhed, ſo well as the *Nautilus*. It is true, it has not ſo many chambers as others of that genus, nor are the apertures of the ſepta of a *tubular* form; but as theſe, according to the laws of method, are to be conſidered as marks of a *ſpecific* rather than *generic* nature, ſo I ſhall not heſitate to refer the ſhell under conſideration to the family of *Nautilus*, at leaſt till we are authoriſed, by the diſcovery of many more of a ſimilar ſtructure, to rank it under a new genus.

That I may give a more full and ſpecific deſcription of this ſingular ſhell, it muſt be obſerved, that its figure is a flatted ſpiral, umbilicated on one ſide, convex on the other, but yet ſlightly depreſſed in the centre, meaſuring in diameter about a quarter of an inch; that it is generally coiled up into four volutions, which are convex above, and ſo nearly plane beneath as to form an acute or carinated margin; and that each of theſe volutions, on the upper ſide, has a narrow thread-like border or fillet on the interior edge. The front view of the mouth is obliquely ſemioval, the upper edge projecting farther than the lower.

The ſubſtance of the ſhell is very brittle and pellucid, and, when alive, of a reddiſh brown or cheſnut colour throughout, except about three or four faint white lines, which appear like rays running from the central umbilicus to different and nearly equidiſtant parts of the circumference. Theſe white lines are not ſtraight, but ſhaped each like a ſhort curve, or comma, on the upper ſide, and are nothing elſe but the ſhades of the *ſepta* in the cavity of the ſhell.

Such is its *external* appearance. The internal ſtructure is extremely curious; for the whole cavity is divided into three, four, or five chambers or compartments (according to the age of the ſhell) at nearly equal diſtances, by tranſverſe *ſepta* of a hard white brittle ſemipellucid ſubſtance, reſembling agate or enamelled glaſs. Each of theſe *ſepta* has a triradiated aperture not unlike the Greek capital upſilon, or the Roman Y, inverted, (⅄) through which the animal, by means of its ſoft compreſſible and extenſible nature, eaſily contrives to extrude itſelf, as much as is neceſſary, when in ſearch of food, or in the act of moving from one place to another.

It may not be amiſs here to obſerve, that the *ſepta* above-mentioned are totally foreign, both in *uſe* and *ſtructure*, from what are called *opercula* in other ſhells: I mean thoſe temporary covers or ſtoppers, made uſe of by many teſtaceous animals to cloſe up the mouths of their ſhells, and defend them from injury in their quieſcent ſtate.

The *opercula*, however various in ſubſtance, are always obſerved to be *ſingle*, *imperforate*, *moveable* at the will of the animal, and conſtantly placed, as a ſecurity, in the *mouth*, never in any *other part* of the cavity of the ſhell; whereas the *ſepta*, in the ſubject now before us, are repeatedly conſtructed in ſeveral parts of the cavity, are all of them *perforated*, intimately connected with the ſubſtance of the ſhell, and conſequently *fixed* and *permanent*, as in all the *Nautili.*

And as to the *uſe* of theſe *ſepta*, though I dare not ſay what might be the real intention of nature in their formation, yet it will be no preſumption to affirm, that they could not be deſigned for the ſame purpoſe as *opercula* in other ſhells; not only becauſe they are placed where they cannot anſwer the ſame end, but more eſpecially on account of their open ſtructure, which intirely excludes them from the poſſibility of affording a proper defence to the encloſed animal.

Should it be ſaid, that they only ſerve to point out the different *periods* or *ſtages* of the ſhell's growth, and are nothing elſe but the *limits* or *terminations* of the animal's periodical increaſe, I will not diſpute the opinion; it may perhaps be very true; but ſuppoſing it to be ſo, is it not equally probable, that the tranſverſe *ſepta* in *all* the Nautili are nothing elſe?

But I muſt not conclude my remarks without taking ſome notice of the *inhabitant* of this ſingular ſhell. It appears to be of the *ſlug kind*, but differs from the common *land* ſorts in this reſpect, that the *Antennæ* are *filiform*, and the eyes not placed upon their *ſummits* and *retractile*, but fixed upon the *head* near their baſes, as is probably the caſe in all the truly *aquatic* kinds, at leaſt in all ſuch as I have hitherto examined. The animal is of a ſoft and flexible nature, and grey brown colour, and has a power of extending itſelf out of the ſhell through the aperture of the exterior *ſeptum*; at which time it aſſumes a *triradriated* ſhape, not very diſſimilar from the aperture itſelf, or like an inverted Y (⅄), the thickeſt ray of which is the head and body; one of the lines which form the angle is the tail, and the other is a kind of dorſal ligament, which extends from the back of the animal, through one of the rays of the aperture, and through the whole cavity of the ſhell, and all its *ſepta*, to the centre, as may be ſeen by placing the ſhell between the eye and the light (ſee fig. 3. Tab. I.).

In the conciſe LINNÆAN mode of deſcription this ſhell may be named,

> Nautilus *(lacuſtris)* teſta ſpirali compreſſa umbilicata carinata, anfractibus tribus ſupra convexis contiguis, apertura ſemiovata, ſeptis triradiato-perforatis.
>
> The Freſh-water Nautilus.

I find no author who has taken any notice of this ſhell, except Mr. WALKER, who, in his late curious publication on *Minute Shells*, has deſcribed it under the name of

Helix *lineata* dorſo convexo umbilicata margine acuto; and has given a figure of it in the ſame work, Pl. I. fig. 28.

But this ingenious gentleman is free enough to confeſs, that its *chambered ſtructure* had entirely eſcaped his notice,

otherwiſe

otherwiſe he would doubtleſs not have ranked it among the *Helices*.

The place where the ſhell is to be found, is in deep ditches of clear water, adhering to the roots of *Carices*. It was collected near Upton Church, not far from Eton, in Buckinghamſhire, in the ſpring ſeaſon. Mr. Walker reports it to be found on flags in Hornhill Brooks, in Kent, but very rare.

The figures annexed will explain what I have been deſcribing much better than words.

Fig. 1. (Tab. I.) The ſhell of its natural ſize, with the umbilicated ſide uppermoſt.

2. The ſame with the depreſſed ſide uppermoſt; the dark ſhade in both ſhewing how far the cavity of the ſhell is occupied by the dead animal included.

3. The ſhell magnified with the *depreſſed* ſide uppermoſt, ſhewing the live animal within it, its head and *antennæ* protruded. Here the white lines appear double, being the ſhade of the *ſepta* on both ſides of the ſhell.

5. The ſame magnified with the *umbilicated* ſide uppermoſt, the head and under ſide of the animal appearing to view.

4. The ſame magnified in a perpendicular view, with the mouth in front, but cut away down to the firſt *ſeptum*, in order to ſhew not only the *carina* or keel of the ſhell, but more eſpecially the exact appearance of the triradiated *ſeptum* neareſt the mouth, and in what manner the animal contrives to extrude itſelf through the aperture, the head and tail being accommodated to paſs through two of the parts of the inverted Y (⅄), while the *dorſal ligament* occupies the third.

8. The animal's excrement.

6, 7. Horizontal ſections of the ſhell, in order to ſhew the internal ſtructure, or the appearance of the *ſepta*, when the ſhell

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ſhell is ground down or divided in that direction. Fig. 6. ſhewing the ſhell ground away in part, with it's umbilicated ſide uppermoſt. Fig. 7. the ſame more deeply and evenly ground, with the depreſſed or more convex ſide uppermoſt.

The *ſecond* ſhell I ſhall take notice of has much of the ſame external face with the preceding, and is nearly of the ſame ſize and colour, but materially differs from it in having an uninterrupted cavity from the mouth to the center; that is, *no divided chambers* or *compartments.* This therefore evidently belongs to the genus of *Helix.*

It is ſtrongly umbilicated on one ſide, and almoſt plane on the other, the central wreaths being nearly of equal height, or but ſlightly depreſſed, and deſtitute of that narrow border or fillet mentioned in the preceding ſhell. It conſiſts moſt commonly of three volutions, convex on both ſides, with an obtuſely carinated margin, and ſemioval mouth.

It may be named,

> Helix (*fontana*) teſta compreſſa obtuſè carinata, hinc umbilicata, anfractibus tribus utrinque convexis, apertura ſemiovata.
>
> Fountain Helix.

The figures here given repreſent this ſhell, on both ſides, in its natural and magnified ſtate, ſo that more words to deſcribe it are needleſs.

Fig. 1. (Tab. II.) The ſhell of the natural ſize, with the moſt convex ſide uppermoſt.

2. The ſame, with the umbilicated ſide uppermoſt.

3. The ſhell magnified, the moſt convex ſide uppermoſt.

4. The ſame magnified, the umbilicated ſide uppermoſt.

I do not find that it has been noticed by any author.

It was found in the bottom of a ſpring of clear water, adhering to the under ſide of rotten leaves, near Bullſtrode, in Buckinghamſhire, in the month of April. It has alſo been found in ſome other clear waters in the ſame neighbourhood, but not common.

A *third* ſhell I have to mention is a very minute but curious *Helix* of a ſubconical form, conſiſting of about five convex wreaths, gradually diminiſhing towards the apex. The ſhell is umbilicated at the baſe, and the wreaths are tranſverſely ſurrounded with numerous ſharp-edged rings, which are produced in the middle or back of each wreath into a kind of ſpur, formed of compreſſed and very tender ſpines. The mouth is a ſegment larger than a ſemicircle, but not round enough to conſtitute the ſhell a *Turbo*, to which it is nevertheleſs nearly allied. The colour of the whole ſhell is brown.

It may be named,

> Helix (*ſpinuloſa*) teſta ſubconica umbilicata, anfractibus 5 convexis, annulis membranaceis acutis cinctis, dorſo ſpinuloſo-carinatis, apertura ſuborbiculari.
>
> Tender prickly Helix.

The figures here given repreſent this ſhell in different poſitions, in its natural and magnified ſtate.

Fig. 1. 2. (Tab. II.) The ſhell, in different poſitions, of the natural ſize.

3. 4. 5. The ſame magnified.

I know no author who has hitherto noticed it.

It was found near Bullſtrode, at the foot of pales, upon old bricks and ſtones, after rainy weather, in June and July.

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A *fourth* is a minute ſhell of the *Turbo* kind.

It ſtrongly reſembles the depreſſed *Helices*; but its circular mouth forbids its being ranked in that *genus*.

It conſiſts of four cylindric or rounded volutions, of nearly equal height on one ſide, but ſunk or umbilicated on the other. Theſe volutions are tranſverſely ſurrounded with numerous ſharp-edged membranaceous rings, which are very fragile and deciduous. The mouth, when perfect, is bordered with a compreſſed erect margin. The colour of the ſhell is uniformly brown.

It may be named,

Turbo (*helicinus*) teſta depreſſo-plana, hinc umbilicata, anfractibus 4 toroſis, annulis numeroſis acutis membranaceis cinctis.

The fine-ringed Turbo.

The figures herewith exhibit both ſides of the ſhell, in its natural and magnified ſtate.

Fig. 1. 2. (Tab. III). The ſhell, on both ſides, of the natural ſize.

3. 4. The ſame, on both ſides, magnified.

No author, that I know of, has deſcribed it.

It was found near Bullſtrode, upon bare ſtones, in the ſpring ſeaſon, and at other times in moiſt weather.

The *fifth* and *laſt* ſhell I have to mention, is a ſmall thin oblong compreſſed *Patella*, of a horn colour, about a quarter of an inch long, and one-tenth of an inch wide, having a pointed vertex neareſt to the lower end, turned downwards, and leaning to one ſide.

It may be called,

Patella (*oblonga*) teſta integerrima oblonga compreſſa membranacea, vertice mucronato reflexo obliquo.

Oblong freſh-water Patella.

It is perfectly diſtinct from the *Patella lacuſtris* of LINNÆUS both in ſhape, and flexure of the vertex, as well as being deſtitute of radiated ſtreaks.

Fig. 1. 2. 3. and 4. (Tab. III.) The natural ſize in different attitudes.

5. A ſhell magnified, with its vertex upward.

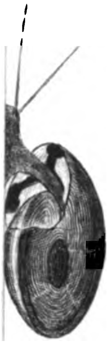
6. Patella *lacuſtris* LIN. ſhewing the plan of the two different ſpecies.

It has eſcaped the notice of all the authors I am acquainted with.

It was found adhering to the leaves of the *Iris Pſeudacorus* in waters near Beaconsfield, in Buckinghamſhire, by Mr. AGNEW, Gardener to the late Ducheſs Dowager of PORTLAND; by whoſe ſagacity all the preceding ſhells were diſcovered, and by whoſe faithful pencil they were drawn.

I have now done with deſcribing the ſhells I intended; but before I conclude, it may not be thought, perhaps, quite foreign to my preſent ſubject, to remove, in ſome degree, an error which has been almoſt univerſally adopted by the dealers and collectors in ſhells, reſpecting certain ſubjects, brought from Jamaica, and other parts of the Weſt-Indies, commonly known by the name of *Gold Shells*. They are yellow gloſſy ſubſtances, of an obtuſely conical figure, and ſize of tares or vetch-ſeeds, compoſed of ſeveral concave brittle imbricated ſcales, cloſely compacted, ſo as to reſemble the foliaceous gem or bud of ſome tree, and have generally a hole or perforation in ſome part. Theſe are commonly ſuppoſed to be *ſhells*, or the

2.



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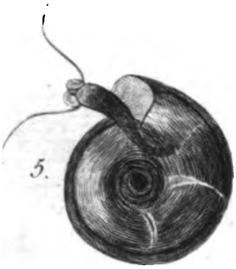

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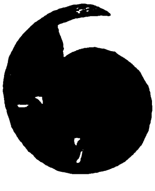

es lacustris.

Basire Sc.

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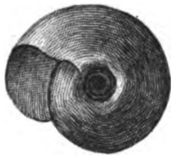
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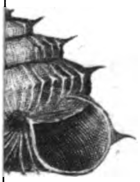
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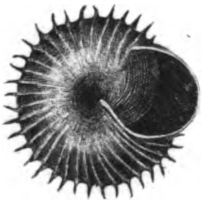
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pinulosa.

J. Pyiro Sc.

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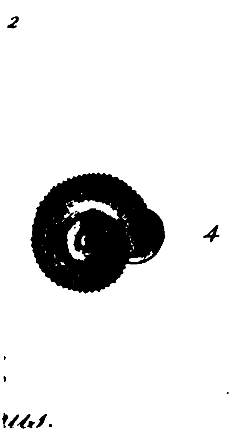
Philos. Trans. Vol. LXXVI. Tab. III. p. 158.



llus.

nga.

Basire Sc.

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embryos of ſhells taken out of ſome bag or ovary. It is certain, however, that this is a miſtake; for having collected a few of the largeſt and moſt opaque of theſe *ſuppoſed ſhells*, and ſuch as had *no* perforation to be found in them, I immerſed them for a few minutes in hot water, and then carefully developing the ſcales of which they were compoſed, I found in the centre of all the largeſt and moſt perfect a ſmall inſect, enveloped in a mealy ſubſtance, about the ſize of a ſmall bed-bug, of a roundiſh oval figure, dark brown colour, convex on the back, ſlightly concave beneath, and in every inſtance, except *one* (out of at leaſt fifty which I opened), all without wings. The body was compoſed of about eight imbricated ſegments or rings; the head was very ſhort, and almoſt concealed under the margin of the thorax; however, I plainly diſcerned, in ſome of the ſpecimens, that it was furniſhed with two ſhort filiform *antennæ*. The trunk had ſix legs; the feet terminated each with a ſharp red claw. The body of the *ſingle* ſpecimen which had *wings* was oblong, and narrower than the *apterous* ones. The wings appeared to be glued down to the body, juſt as in a bee or waſp, when it is almoſt ready to emerge from the *Pupa* ſtate. Whether they were two or four wings I am not abſolutely certain; but they appeared to be of the filmy tranſparent kind, at leaſt near the extremities; for I clearly perceived the nerves as in the wings of a fly. From hence it evidently appears, that theſe *Gold Shells* are really no other than the *caſes* or cells of an inſect in its *Pupa* ſtate; and from conſidering the *form of its body*, the *difference of the ſexes*, the one being *apterous*, the other *winged*, I have no doubt but it is a ſpecies of cochineal or *coccus*, and probably one not hitherto deſcribed by naturaliſts. The *caſes* do not efferveſce with acids, therefore they are not of a *teſtaceous* nature. They ſeem to be a vege-

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table ſubſtance of the *reſinous* kind; for they bubble a little on being burnt on a hot iron, and when triturated diſſolve ſlowly in a warm ſpirituous menſtruum to a ſweet-ſmelling viſcid matter. But we muſt wait for a better elucidation of the ſubject from thoſe who collect theſe ſubſtances in their native place.

I have the honour to be, with the utmoſt reſpect, &c.

JOHN LIGHTFOOT.

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VIII. *Obſervations on the Sulphur Wells at* Harrogate, *made in* July *and* Auguſt, 1785. *By the Right Reverend* Richard *Lord Biſhop of* Landaff, *F. R. S.*

Read February 2, 1786.

IN 1733, when Doctor SHORT firſt publiſhed his Treatiſe on Mineral Waters, there were only three ſulphur wells at Harrogate; there are now four. I made ſome inquiry reſpecting the time and occaſion of making the fourth well, and received the following account from an old man, who was himſelf principally concerned in the tranſaction. About forty years ago, a perſon who, by leaſe from the Earl of BURLINGTON, had acquired a right of ſearching for minerals in the foreſt of Knareſborough, made a ſhew as if he had a real intention of digging for coal, on the very ſpot where the three ſulphur wells were ſituated. This attempt alarmed the apprehenſions of the inn-keepers and others at Harrogate, who were intereſted in the preſervation of the wells: they gave him what legal oppoſition they could, and all the illegal that they durſt. At length, for the ſum of one hundred pounds, which they raiſed amongſt themſelves, the diſpute was compromiſed, and the deſign real or pretended of digging for coal was abandoned. Sulphur water, however, had riſen up where he had begun to dig. They incloſed the place with a little ſtone edifice, and putting down a baſon, made a fourth well. By a clauſe in the act of parliament for incloſing Knareſborough Foreſt, paſſed in 1770, it is rendered unlawful for any perſon whatever to ſink any pit, or dig any quarry or mine, whereby the medicinal ſprings or waters at Harrogate may be damaged or polluted; ſo that no attempts of the kind above-mentioned need be apprehended in future.

This fourth well is that which is neareſt to one of the barns of the Crown-Inn, being about ten yards diſtant from it. In digging, a few years ſince, the foundation of that barn, they met with ſulphur water in ſeveral places. At a very little diſtance from the four wells there are two others of the ſame kind; one in the yard of the Half-Moon-Inn, diſcovered in digging for common water in 1783, and another which breaks out on the ſide of the rivulet below that Inn. On the banks of that rivulet I ſaw ſeveral other ſulphureous ſprings: they are eaſily diſtinguiſhed by the blackneſs of the earth over which they flow.

On the declivity of a hill, about a quarter of a mile to the weſt of the ſulphur wells at Harrogate, there is a bog which has been formed by the rotting of wood: the earth of the rotten wood is in ſome places four feet in thickneſs, and there is a ſtratum conſiſting of clay, and ſmall looſe decaying ſand-ſtones, every where under it. The hill above is of grit-ſtone. In this bog there are four more ſulphur wells; one at the top, near the rails which ſeparate the bog from the Common; and three at the bottom, though one of theſe, ſtrictly ſpeaking, is not in the bog but at the ſide of it in the ſtratum on which the bog is ſituated, and at the diſtance of a yard or two from a rivulet of freſh water, which runs from thence to Low Harrogate, paſſing cloſe to the ſide but above the level of the ſulphur wells of that place. On the other ſide of the hill, above the bog, and to the weſt of it, there is another ſulphur well on the ſide of a brook; and it has been thought that the wells both at Harrogate and in the bog are ſupplied from this well. In a low ground, between High Harrogate and Knareſborough, there is a ſulphur well; another to the north of it in Bilton Park, at about the diſtance of a mile; and another to the ſouth of it, at a leſs diſtance, was diſcovered this year in digging for common water by a perſon of the name of RICHARDSON; and, laſtly, there is another at a place called Hookſtone Crag: none of theſe laſt mentioned wells are above two miles diſtant from High Harrogate; and by an accurate ſearch a great many more might, probably, be diſcovered in the neighbourhood.

It is not unuſual to dig within a few yards of any of theſe ſulphur wells, and to meet with water which is not ſulphureous. I ordered a well to be dug in the fore-mentioned bog, ſixteen yards to the ſouth of the ſulphur well which is near the rails, and to the ſame depth with it; the water with which it was preſently filled was chalybeate, but in no degree ſulphureous. I had another well dug, at about thirty yards diſtance from the three ſulphur wells which are ſituated at the lower extremity of the bog; this well, by the declivity of the ground, was ten or twelve feet below their level, but its water was not ſulphureous. From the firſt well which I dug, it is evident, that every part of the bog does not yield ſulphur water; and from the ſecond, which was ſunk into the clay, it is clear that every part of the ſtratum on which the bog is placed does not yield it, though one of the wells is ſituated in it.

The ſulphur wells at Harrogate are a great many feet below the level of thoſe in the bog; but they communicate with them, if we may rely on what Doctor SHORT has told us — "That about the beginning of this century, when the concourſe of people was very great to the Spaw at Harrogate, one ROBERT WARD, an old man, made a baſon in the clay under the moſs of a bog where the ſtrongeſt and briſkeſt of theſe ſulphur ſprings riſe, and gathered half an hogſhead of water at a time for the uſe of the poor; but when he laded this he almoſt dried the three ſulphur wells at the village, whence it is evident, that all have the ſame origin and communicate with one another." By converſing with ſome of the oldeſt and moſt intelligent people at Harrogate, I could not find that they entertained any opinion of the water at the bog having a communication with that at the ſpaw. This circumſtance might eaſily be aſcertained; and, if the fact ſhould be contrary to what Doctor SHORT ſuppoſed, the wells at the bog ought to be covered from the weather as thoſe at the village are; they would by this mean yield great plenty of water for the baths which are wanted by invalids, and which are often very ſcantily ſupplied by the wells at Harrogate, notwithſtanding the attention which is uſed in preſerving the water which ſprings at the four wells, by emptying them as often as they become full during both the day and night time. And indeed it is ſurpriſing, that the well on the ſide of the rivulet below the Half-Moon-Inn, which is ſo well ſituated for the purpoſe, has never been incloſed for the furniſhing ſulphureous water for the baths. The preſent mode of carrying the water in caſks to the ſeveral houſes where the perſons lodge who want to bathe in it, is very troubleſome, and the water thereby loſes of its virtue. Some of the wells about the village, that for inſtance which has been diſcovered at the Half-Moon-Inn, the water of which, I believe, ſprings from a different ſource from that which ſupplies the four ſulphur wells, ſhould be either enlarged to a greater horizontal breadth, or ſunk to a greater depth, in order to try, by one or both of theſe ways, whether the quantity and ſtrength of the water might not be increaſed;

and if that ſhould, as it probably would be the caſe, one or more baths might be erected after the manner of thoſe at Buxton and other places; or, by proper additional buildings, warm bathing in ſulphureous water might be practiſed, as is done in common water in the bagnios in London. The ſaltneſs of the ſulphureous water, if that ſhould be thought uſeful, might eaſily be made even greater than that of ſea water, by adding a quarter of a pound of common ſalt to every gallon of the water uſed in forming a bath. The waters at Harrogate, though they have long been very beneficial, have not yet been rendered ſo uſeful to mankind, as an intelligent and enterpriſing perſon might make them. The alternate ſtrata of ſand, ſtone, and ſhale, which compoſe the lower hills near the wells at Harrogate, dip very much, as may be ſeen in a ſtone quarry about two hundred yards from the wells; and the ſame circumſtance may be obſerved in dry weather, in following the bottom of the brook from the village up to the bog; and hence, if there be a communication between the waters of the bog and of the village, as Doctor SHORT aſſerts, it is probable, that the ſame ſtratum of ſhale which is ſeen at the bottom of the wells at the village, breaks out again at the bog above the village, and that the water finds its way from the bog to the village through the crevices of that ſtratum.

After having obſerved, as carefully as I could, the number and ſituation of the ſulphur wells about Harrogate, I took notice of the temperature of the four at the village. In the month of June, 1780, when the thermometer in the ſhade was 72°, and the pump water at the Granby-Inn, the well of which is fifty feet deep, was 48°, the ſtrongeſt of the ſulphur wells, being that of which invalids uſually drink, was 50°. On the 29th of July in this year, after the earth had been parched with drought for many months, the heat of the ſtrongeſt well was 54°; the water of the Granby pump was on the ſame day 48°, and the heat of the air in the ſhade 76°. Doctor WALKER, who has lately written a treatiſe on Harrogate water, ſays, that the heat of this ſpring was 48°, when that of an adjoining rivulet was 53°. And I have little doubt in believing, that if the experiment was made in cold weather, the temperature of the ſame well would be found to be ſeveral degrees below 48. This variation of temperature in the ſulphur water indicates its ſpringing from no great depth below the ſurface of the earth; or at leaſt it indicates its having run for a conſiderable diſtance in a channel ſo near to the ſurface of the earth, as to participate of the changes of temperature, to which that is liable from the action of the ſun. But the heat of the ſulphur water is not only variable in the ſame well, at different times, but it is not the ſame in all the wells at the ſame time. If we call the ſtrongeſt well the firſt, and reckon the reſt in order, going to the right, the third well, which is reckoned the next ſtrongeſt, was 57° hot when the firſt well was 54°. In ſupport of the conjecture that the ſulphur water of the ſtrongeſt well would in a cold ſeaſon make the thermometer ſink below 48°, which is the conſtant temperature of ſprings ſituated at a great depth in the earth in this country, it may be obſerved, that though the firſt and the third well are never frozen, yet the ſecond and the fourth well are frozen in ſevere weather. When the ſecond and the fourth well are covered with ice, it is probable, that the firſt and the third have a temperature far below 48°; but that the ſea ſalt, which is more abundant in them than in the other two wells, and which of all ſalts reſiſts moſt powerfully the congelation of the water in which it is diſſolved, preſerves them from being frozen in the coldeſt ſeaſons incident to our climate.

As the temperature of theſe four wells is not the ſame in all of them at the ſame time, nor invariable in any of them, ſo neither does there ſeem to be any uniformity or conſtancy in them, with reſpect to the quantity of ſalt which they contain. The ſalt with which they are all impregnated is of the ſame kind in all, and it is almoſt wholly common ſalt; and though the quantity contained in a definite portion of any one of the wells is not, I think, preciſely the ſame at all ſeaſons of the year, yet the limits within which it varies are not, I apprehend, very great. A method is mentioned in the LXth volume of the Philoſophical Tranſactions, of eſtimating the quantity of common ſalt diſſolved in water, by taking the ſpecific gravity of the water: this method is not to be relied on, when any conſiderable portion of any other kind of ſalt is diſſolved along with the ſea ſalt; but it is accurate enough to give a good notion of the quantity contained in the different wells at Harrogate. On the 13th of Auguſt, after ſeveral days of rainy weather, I took the ſpecific gravities of the four ſulphur wells at the village, the drinking well being the firſt.—Rain water 1.000; firſt well 1.009; ſecond well 1.002; third well 1.007; fourth well 1.002. By comparing theſe ſpecific gravities with the table which is given in the LXth volume of the Tranſactions, it may be gathered, that the water of the firſt well contained $\frac{1}{78}$ of its weight of common ſalt; that of the ſecond and fourth, $\frac{1}{258}$; and that of the third, $\frac{1}{84}$. After four days more heavy rain I tried the ſtrongeſt well again, and found its ſpecific gravity to be 1.008. It is worthy of obſervation, that the water, as it ſprings into the firſt and third well, is quite tranſparent, but uſually of a pearl colour in the ſecond and

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fourth, ſimilar in appearance to the water of the firſt or third well after it has been expoſed a few hours to the air; hence it is probable, that the external air has acceſs to the water of the ſecond and fourth well before it ſprings up into the baſon. A great many authors have publiſhed accounts of the quantity of common ſalt contained in a gallon of the water of the ſtrongeſt well; they differ ſomewhat from each other, ſome making it more, others leſs, than two ounces. Theſe diverſities proceed either from the different care and ſkill uſed in conducting the experiment; or from a real difference in the quantity of ſalt with which the water is impregnated at different ſeaſons of the year. The medium quantity of ſalt contained in a gallon falls ſhort of, I think, rather than exceeds two ounces. The ſea water at Scarborough contains about twice as much ſalt as is found in the ſtrongeſt ſulphur well at Harrogate. The ſulphur wells at the bog are commonly ſaid to be ſulphureous, but not ſaline. This, however, is a miſtake; they contain ſalt, and ſalt of the ſame kind as the wells at the village. I could not diſtinguiſh the kind of ſalt by the method in which I had eſtimated the quantity contained in the ſulphur wells; I therefore evaporated a gallon of the water of the well in the bog which is near the rails, and obtained a full ounce of common ſalt, of a browniſh colour: the colour would have gone off by calcination. In what degree the medicinal powers of Harrogate water depend on its ſulphureous, and in what degree on its ſaline impregnation, are queſtions which I meddle not with: I would only juſt obſerve on this head, that any ſtrong ſulphureous water, ſuch as that of Keddleſtone in Derbyſhire, or of Shap in Weſtmoreland, which naturally contains little or no ſea ſalt, may be rendered ſimilar to Harrogate water, by diſſolving in it a proper proportion of common ſalt. The four ſulphur wells at Harrogate are very near to each other; they might all be included within the circumference of a circle of ſeven or eight yards in diameter; yet, from what has been ſaid it is evident, that they have not all either the ſame temperature, or the ſame quantity of ſaline impregnation. This diverſity of quality, in wells which have a proximity of ſituation, is no uncommon phænomenon; and though at the firſt view it ſeems to be ſurpriſing, yet it ceaſes to be ſo on reflexion: for the waters which feed wells ſo circumſtanced, may flow through ſtrata of different qualities ſituated at different depths, though in the ſame direction; or through ſtrata placed both at different depths, and in different directions; and that this is the caſe at Harrogate is probable enough, there being hills on every ſide of the hollow in which the village is placed.

With reſpect to the ſulphureous impregnation of theſe waters, I made the following obſervations.

The inſide of the baſon, into which the water of the ſtrongeſt well riſes, is covered with a whitiſh pellicle, which may be eaſily ſcraped off from the grit-ſtone of which the baſon is made. I obſerved, in the year 1780, that this pellicle on a hot iron burned with the flame and ſmell of ſulphur. I this year repeated the experiment with the ſame ſucceſs; the ſubſtance ſhould be gently dried before it is put on the iron. I would further obſerve, that the ſulphur is but a ſmall part of the ſubſtance which is ſcraped off. That I might be certain of the poſſibility of obtaining true palpable ſulphur from what is ſcraped off from the baſon, and at the ſame time give ſome gueſs at the quantity of ſulphur contained in it, I took three or four ounces of it, and having waſhed it well, and dried it thoroughly by a gentle heat, I put two ounces into a clean glaſs retort, and ſublimed from it about two or three grains of yellow ſulphur. This ſulphur, which ſtuck to the neck of the retort, had an oily appearance; and the retort, when opened, had not only the ſmell of the volatile ſulphureous acid, which uſually accompanies the ſublimation of ſulphur, but it had alſo the ſtrong empyreumatic ſmell which peculiarly appertains to burnt oils; and it retained this ſmell for ſeveral days. It has been remarked before, that the ſalt ſeparable from the ſulphur water was of a browniſh colour; and others, who have analyſed this water, have met with a brown ſubſtance, which they knew not what to make of; both which appearances may be attributed to the oil, the exiſtence of which was rendered ſo manifeſt by the ſublimation here mentioned. I will not trouble the Society with any conjectures concerning the origin of this oil, or the medium of its combination with water; the diſcovery of it gave me ſome pleaſure, as it ſeemed to add a degree of probability to what I had ſaid concerning the nature of the air with which, in one of my Chemical Eſſays, I had ſuppoſed Harrogate water to be impregnated. I will again take the liberty of repeating the query which I there propoſed. "Does this air, and the inflammable air ſeparable from ſome metallic ſubſtances, conſiſt of *oleaginous* particles in an elaſtic ſtate?" When I ventured to conjecture, in the Eſſay alluded to, that ſulphureous waters received their impregnation from air of a particular kind, I did not know that Profeſſor BERGMAN had advanced the ſame opinion, and denominated that ſpecies of air, Hepatic Air. I have ſince then ſeen his works, and very readily give up to him not only the priority of the diſcovery, but the merit of proſecuting it. And though what he has ſaid concerning the manner of precipitating ſulphur from theſe waters can leave no doubt in the mind of any chemiſt concerning the actual exiſtence of ſulphur in them; yet I will proceed to the mention of ſome other obvious experiments on the Harrogate water, in ſupport of the ſame doctrine.

Knowing that, in the baths of Aix-la-Chapelle, ſulphur is found ſticking to the ſides and top of the channel in which the ſulphureous water is conveyed, I examined with great attention the ſides of the little ſtone building which is raiſed over the baſon of the ſtrongeſt well, and ſaw them in ſome places of a yellowiſh colour: this I thought proceeded from a ſpecies of yellow moſs, commonly found on grit-ſtone: I collected, however, what I could of it by bruſhing the ſides of the building, at the diſtance of three or four feet from the water in the baſon: on putting what I had bruſhed off on a hot iron, I found that it conſiſted principally of particles of grit-ſtone, evidently however mixed with particles of ſulphur.

Much of the ſulphureous water is uſed for baths at Harrogate; and for that purpoſe all the four wells are frequently emptied into large tubs containing many gallons apiece; theſe conſtantly ſtand at the wells, and the caſks, in which the water is carried to the ſeveral houſes, are filled from them. On examining the inſides of theſe tubs, I found them covered, as if painted, with a whitiſh pellicle. I ſcraped off a part of this pellicle: it was no longer ſoluble in water; but, being put on a hot iron, it appeared to conſiſt almoſt wholly of ſulphur. Some of theſe tubs have been in uſe many years, and the adhering cruſt is thick in proportion to the time they have been applied to the purpoſe; but the ſulphur pellicle was ſufficiently obſervable on one which was new in the beginning of this ſeaſon. The water when it is firſt put into theſe tubs is tranſparent; when it has been expoſed to the air for a few hours, it becomes milky; and, where the quantity is large, a white cloud may be ſeen ſlowly precipitating itſelf to the bottom. This white precipitate conſiſts partly, I am not certain that it conſiſts wholly, of ſulphur; and the ſulphur is as really contained in the waters denominated ſulphureous, as iron is contained in certain ſorts of chalybeate waters; in the one caſe the iron is rendered ſoluble in water by its being united to fixed air, or ſome other volatile principle; and in the other ſulphur is rendered ſoluble in water by its being united to fixed air, or ſome other volatile principle: neither iron nor ſulphur are of themſelves ſoluble in water, but each of them, being reduced into the form of a ſalt by an union with ſome other ſubſtances, becomes ſoluble in water, and remains diſſolved in it, till that other ſubſtance either eſcapes into the air, or becomes combined with ſome other body.

About forty years ago, they took up the baſon of the third well, and a credible perſon, who was himſelf preſent at the operation, informed me, that in all the crevices of the ſtone on which the baſon reſted, there were layers of pure yellow ſulphur. This I can well believe, for I ordered a piece of ſhale to be broken off from the bottom of the fourth well; it was ſplit, as ſhale generally is, into ſeveral thin pieces, and was covered with a whitiſh cruſt. Being laid on a hot iron, in a dark room, it cracked very much, and exhibited a blue flame and ſulphureous ſmell.

If the water happens to ſtand a few days in any of the wells, without being diſturbed, there is found at the bottom a black ſediment; this black ſediment alſo marks the courſe of the water which flows from the well, and it may be eſteemed characteriſtic of a ſulphur water. The ſurface of the water alſo, when it is not ſtirred for ſome time, is covered with a whitiſh ſcum. Doctor SHORT had long ago obſerved, that both the black ſediment, and the white ſcum, gave clear indications, on a hot iron, of their containing ſulphur: I know not whence it has come that his accuracy has been queſtioned in this point; certain I am, that on the repetition of his experiments I found them true. The white ſcum alſo, which is found ſticking on the graſs over which the water flows, being gently dried, burns with the flame and ſmell of ſulphur. From what has been ſaid it is clear, that ſulphur is found at Harrogate, ſticking to the baſon into which the water ſprings; ſublimed upon the ſtones which compoſe the edifice ſurrounding the well; adhering to the ſides of the tubs in which the water ſtands; ſubſiding to the bottom of the channel in which the water runs; and covering the ſurface of the earth, and of the blades of graſs, over which it flows. It is unneceſſary to add another word on this ſubject; it remains that I riſk a conjecture or two, on the primary cauſe of the ſulphureous impregnation obſervable in theſe waters.

In the Chemical Eſſay before referred to, I have ſhewn, that the air ſeparable from the lead ore of Derbyſhire, or from Black-Jack, by ſolution in the acid of vitriol, impregnates common water with the ſulphureous ſmell of Harrogate water; and I have alſo ſhewn that the bladder fucus or ſea-wrack, by being calcined to a certain point, and put into water, not only gives the water a brackiſh taſte, but communicates to it, without injuring its tranſparency, the ſmell, taſte, and other properties of Harrogate water. Profeſſor BERGMAN impregnated water with a ſulphureous taſte and ſmell, by means of air ſeparated by the vitriolic acid from hepar ſulphuris, made by fuſion of equal weights of ſulphur and pot-aſhes, and from a maſs made of three parts of iron filings melted with two of ſulphur; and he found alſo, that Black-Jack and native Siberian iron yielded hepatic air, by ſolution in acids. This, I believe, is the main of what is known by chemiſts on this ſubject; what I have to ſuggeſt, relative to the Harrogate waters in particular, may perhaps be of uſe to future inquirers.

I have been told, that on breaking into an old coal-work, in which a conſiderable quantity of wood had been left rotting for a long time, there iſſued out a great quantity of water ſmelling like Harrogate water, and leaving, as that water does, a white ſcum on the earth over which it paſſed. On opening a well of common water, in which there was found a log of rotten wood, an obſervant phyſician aſſured me, that he had perceived a ſtrong and diſtinct ſmell of Harrogate water. Dr. DARWIN, in his ingenious Account of an artificial Spring of Water, publiſhed in the firſt part of the LXXVth volume of the Philoſophical Tranſactions, mentions his having perceived a ſlight ſulphureous ſmell and taſte in the water of a well which had been ſunk in a black, looſe, moiſt earth, which appeared to have been very lately a moraſs, but which is now covered with houſes built upon piles. In the bog or moraſs above-mentioned there is great plenty of ſulphureous water, which ſeems to ſpring from the earth of the rotten wood of which that bog conſiſts. Theſe facts are not ſufficient to make us certain, that rotten wood is efficacious in impregnating water with a ſulphureous ſmell; becauſe there are many bogs in every part of the world, in which no ſulphureous water has ever been diſcovered. Nor, on the other hand, are they to be rejected as of no uſe in the inquiry; becauſe wood, at a particular period of its putrefaction, or when ſituated at a particular depth, or when incumbent on a ſoil of a particular kind, may give an impregnation to water, which the ſame wood, under different circumſtances, would not give.

The bilge water, uſually found at the bottom of ſhips which are foul, is ſaid to ſmell like Harrogate water: I at firſt ſuppoſed, that it had acquired this ſmell in conſequence of becoming putrid in contact with the timber on which it reſted, and this circumſtance I conſidered as a notable ſupport to the conjecture I had formed of rotten wood being, under certain circumſtances, inſtrumental in generating the ſmell of Harrogate water. But this notion is not well founded; for the bilge water is, I ſuppoſe, ſalt water; and Dr. SHORT ſays, that ſea water, which had been kept in a ſtone bottle ſix weeks "ſtunk not much ſhort of Harrogate ſulphur water." It has been remarked above, that calcined ſea-wrack, which contains a great deal of ſea ſalt, exhales an odour ſimilar in all reſpects to that of Harrogate water; and in confirmation of the truth of this remark, I find that an author, quoted by Dr. SHORT, ſays, that "Bay ſalt thrice calcined, diſſolved in water, gives exactly the odour of the ſulphur Well at Harrogate." From theſe experiments conſidered together, it may, perhaps, be inferred, that common ſalt communicates a ſulphureous ſmell to water both by putrefaction and calcination. Hence ſome may think, that there is ſome probability in the ſuppoſition, that either a calcined ſtratum of common ſalt, or a putreſcent ſalt ſpring, may contribute to the production of the ſulphureous ſmell of Harrogate water; eſpecially as theſe waters are largely impregnated with common ſalt. However, as neither the ſalt in ſea water, nor that of calcined ſea-wrack, nor calcined bay ſalt, are any of them abſolutely free from the admixture of bodies containing the vitriolic acid, a doubt ſtill remains, whether the ſulphureous exhalation, here ſpoken of, can be generated from ſubſtances in which the vitriolic acid does not exiſt.

The ſhale from which alum is made, when it is firſt dug out of the earth, gives no impregnation to water; but by expoſure to air and moiſture its principles are looſened, it ſhivers into pieces, and finally moulders into a kind of clay, which has an aluminous taſte. Alum is an earthy ſalt reſulting from an union of the acid of ſulphur with pure clay; and hence we are ſure, that ſhale, when decompoſed by the air, contains the acid of ſulphur; and from its oily black appearance, and eſpecially from its being inflammable, we are equally certain that it contains phlogiſton, the other conſtituent part of ſulphur. And indeed pyritous ſubſtances, or combinations of ſulphur and iron, enter into the compoſition of many, probably of all ſorts of ſhale, though the particles of the pyrites may not be large enough to be ſeen in ſome of them; and if this be admitted, then we need be at no loſs to account for the bits of ſulphur, which are ſublimed to the top of the heaps of ſhale, when they calcine large quantities of it for the purpoſe of making alum: nor need we have any difficulty in admitting, that a phlogiſtic vapour muſt be diſcharged from ſhale, when it is decompoſed by the air. Dr. SHORT ſays, that he burned a piece of aluminous ſhale for half an hour in an open fire; he then powdered and infuſed it in common water, and the water ſent forth a moſt intolerable ſulphureous ſmell, the very ſame with Harrogate water. He burned ſeveral other pieces of ſhale, but none of them ſtunk ſo ſtrong as the firſt. This difference may be attributed, either to the different qualities of the different pieces of ſhale which he tried, or to the calcination of the firſt being puſhed to a certain definite degree; for the combination of the principles on which the ſmell depends may be produced by one degree of heat, and deſtroyed by another. I have mentioned, briefly, theſe properties of ſhale, becauſe there is a ſtratum of ſhale extended over all the country in the neighbourhood of Harrogate; ſeveral beds of it may be ſeen in the ſtone quarry above the ſulphur wells; many of the brooks about Harrogate run upon ſhale, and the ſulphur wells ſpring out of it. They have bored to the depth of twenty yards into this ſhale, in different places, in ſearch of coal, but have never penetrated through it. Its hardneſs is not the ſame at all depths. Some of it will ſtrike fire, as a pyrites does, with ſteel; and other beds of it are ſoft, as if in a ſtate of decompoſition, and the ſulphur water is thought to riſe out of that ſhale which is in the ſofteſt ſtate. But whatever impregnation ſhale when calcined, or otherwiſe decompoſed to a particular degree, may give to the water which paſſes over it; it muſt not be concluded, that ſhale in general gives water a ſulphureous impregnation; ſince there are many ſprings, in various parts of England, ariſing out of ſhale, in which no ſuch impregnation is obſerved.

I forgot to mention, in its proper place, that having viſited the bog, ſo often ſpoken of, after a long ſeries of very dry weather, I found its ſurface, where there was no graſs, quite candied over with a yellowiſh cruſt, of tolerable conſiſtency, which had a ſtrong aluminous taſte, and the ſmell of honey. **Bergman** ſpeaks of a turf found at Helſingberg in Scania, conſiſting of the roots of vegetables, which was often covered with a pyritous cuticle, which, when elixated, yielded alum; and I make no doubt, that the Harrogate moraſs is of the ſame kind.

Whether nature uſes any of the methods which I have mentioned of producing the air by which ſulphureous waters are impregnated, may be much queſtioned; it is of uſe, however, to record the experiments by which her productions may be imitated; for though the line of human underſtanding will never fathom the depths of divine wiſdom, diſplayed in the formation of this little globe which we inhabit; yet the impulſe of attempting an inveſtigation of the works of God is irreſiſtible; and every phyſical truth which we diſcover, every little approach which we make towards a comprehenſion of the mode of his operation, gives to a mind of any piety the moſt pure and ſublime ſatisfaction.

IX. *Obſervations and Remarks on thoſe Stars which the Aſtronomers of the laſt Century ſuſpected to be changeable.* By Edward Pigott, *Eſq.*; *communicated by Sir* Henry C. Englefield, Bart. *F. R. S. and A. S.*

Read February 9, 1786.

IT is about a century ſince HEVELIUS, MONTANARI, FLAMSTEED, MARALDI, and CASSINI, noticed a certain number of ſtars which they ſuppoſed had either diſappeared, changed in brightneſs, or were new ones; and yet to this day we have acquired no further knowledge of them. This may be attributed to the difficulty of finding out what ſtar is meant, and the not having exact obſervations of their relative brightneſs. I therefore have drawn up the following catalogue, and made the neceſſary obſervations; ſo that in future we can examine them without much trouble, and be certain of any change that may take place. To accompliſh this, it was requiſite to compare with attention many authors and moſt of the catalogues of ſtars; in doing which I have perceived ſeveral undoubted errors, and others highly probable; but as entering into a diſcuſſion of ſuch diſagreements would ſwell this account conſiderably, and make it very intricate, I ſhall avoid, as much as poſſible, any thing of the kind that is not immediately neceſſary.

In order to ſeparate certainty from doubt, I have claſſed theſe ſtars in two diviſions; the firſt are undoubtedly changeable;

the others remain yet to be better authenticated. Though ſome of them bear all the appearance of being variable, ſtill no *certainty* of their being ſo has come to my knowledge. To thoſe of the firſt claſs are ſubjoined obſervations made on them within theſe laſt four years, from which the period and progreſſive changes of ſome are deduced, though never ſettled before, and if already known are more exactly determined by comparing my obſervations with former ones. Alſo, as the poſition of ſeveral are determined only by ancient aſtronomers, and therefore inaccurately, I have obſerved them with great exactneſs, the declinations being taken with a Bird's eighteen-inch quadrant, and the right aſcenſions with a three-feet tranſit inſtrument: theſe laſt may ſerve in future to diſcover their proper motions in right aſcenſion, for which reaſon I ſhall ſpecify the ſtars to which they were compared. The ſtars of the ſecond claſs have either their relative brightneſs exactly ſettled, or their non-exiſtence aſcertained. I have alſo pointed out the probability of a miſtake in ſeveral, and in general given an account of the appearance they have had within theſe few years.

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Catalogue of variable Stars, reduced to the beginning of 1786.

Claſs the firſt.

Names.	R. A. in time.			Declination.				Greateſt and leaſt magnitudes.	From whence reduced.
	h.	′	″	°	′	″			
Nova 1572, in Caſſiopea, ...	00	13	00 −	62	58	+	N	1 — 0	Ricciolus's Almageſtum, &c.
ο Ceti	2	8	33	3	57	25	S	2 — 0	Bradley.
Algol	2	54	19	40	6	55	N	2 — 4	Mayer.
Mayer's 420th in Leo	9	36	5	12	25	00	N	6 — 0	Mayer.
In Hydra . . .	13	18	4 +	22	9	38	S	4 — 0	From my obſervat.
Nova 1604 in Serpentarius . .	17	18	00 −	21	10	½	S	1 — 0	Phil. Tr. N° 346.
β Lyræ	18	42	11	33	7	46	N	3 — 4 . 5	Bradley.
Near the Swan's head	19	38	58	26	48	½	N	3 — 0	Phil. Tr. N° 65.
η Antinoi . . .	19	41	34	00	28	14	N	3 . 4 — 5	La Caille.
In the Swan's neck	19	42	21 −	32	22	58	N	5 — 0	From my obſervat.
In the Swan's breaſt	20	9	54	37	22	37	N	3 — 0	From my obſervat.
δ Cephei . . .	22	21	00	57	20	00	N	4 . 3 — 4 . 5	Flamsteed.

Claſs the ſecond.

Hevelius's 6 Caſſiopeæ	00	23	16	60	50	00	N	7 — 0	Hevelius.
46 or ξ Andromedæ	1	9	46	44	24	00	N	4 . 5 — 5 . 6	Flamsteed.
50 or υ Andromedæ	1	24	16	40	20	15	N	4 . 5 — 0	Flamsteed.
Hevelius's 41 Androm.	1	28	40	41	31	¼	N	5 — .	Hevelius.
Tycho's 20th Ceti	1	39	.	13	20	.	S	5 — 0	Tycho.
55 or Neb. Andromedæ	1	40	30	39	40	3	N	6 — .	Flamsteed.
Ptol. and Ul. Beigh ο Eridani . ' .	2	42	.	9	40	.	S	4 — 0	Ul. Beigh.
41 Tauri . . .	3	53	27	27	00	39	N	5 — 0	Flamsteed.
47 Eridani . . .	4	23	54	8	41	40	S	4 — 0	Flamsteed.
Near 53d Eridani .	4	29	00	12	30	±	S	4 — 0	By eſtimation.
γ Canis Majoris.	6	54	5	15	19	36	S	3 — 0	La Caille.
β Geminorum . .	7	32	11 +	28	31	38	N	1 — 3	Maskelyne.
ξ Leonis	9	20	24	12	14	23	N	4 — 6	Mayer.
ψ Leonis . . .	9	32	3	14	59	36	N	5 . 6 — 0	Mayer.
25th Leonis . .	9	46	8	12	20	36	N	6 . 7 — 0	Flamsteed.
Bayer's ι Leonis .	9	52	½	15	30	.	N	6 — 0	Tycho.
δ Urſæ Majoris . .	12	4	45	58	13	24	N	2 — 4	La Caille.

Claſs the ſecond continued.

Names.	R. A. in Time.			Declination.				Greateſt and leaſt magnitudes.	From whence reduced.
	h.	′	″	°	′	″			
η Virginis . . .	12	7	43	00	24	16	N	6 0	Mayer.
Bayer's ✱ near g♍	12	53	00	10	00	.	S	6–0	From maps.
In N. thigh of Virgo	13	29	+	00	30	.	S	6–0	From maps.
91 Virginis . . .	13	48	43	2	5	50	N	6–0	Flamsteed.
α Draconis . . .	13	58	36	65	24	8	N	2–4	Bradley.
In weſt ſcales of Libra	14	53	½	13	26	.	S	6–0	Mém. de l'Acad. des Sciences.
Ptol. and Ul. Beigh's 6th unformed in Libra	15	29	+	20	30	.	S	4–7	Ul. Beigh.
κ Libræ	15	29	39	19	58	27	S	4– .	La Caille.
Tycho's 11th Libræ	15	37	½	19	30	.	S	4–0	Tycho.
33 Serpentis . .	15	38	00	17	14	00	N	6–0	Flamsteed.
Near ε Urſæ Minoris	16	¼	.	82	¾	.	N	6–0	From maps.
Ptol. 14 Ophiuchi	17	2	14	26	15	37	S	4–0	Bradley.
Ptol. 13 Ophiuchi	17	18	+	20	35	.	S	4–0	Ptol.
Ptol. 18 Ophiuchi	17	22	.	24	10	.	S	5–0	Ptol.
σ Sagittarii . . .	18	42	00	26	32	34	S	2–4	Mayer.
θ Serpentis . . .	18	45	35	3	56	26	N	4–5	La Caille.
Tycho's 27th Capricor.	21	41	.	14	28	.	S	6–0	Tycho.
Tycho's 22d Androm.	21	43	½	49	15	.	N	4–0	Tycho.
Tycho's 19 Aquarii	22	25	.	15	55	.	S	6–0	Tycho.
ο Andromedæ . .	22	52	6	41	10	45	N	4–6	La Caille.
La Caille's 483 Zodi. Cat. . .	22	55	40	8	50	45	S	7–0	La Caille.

I ſhall now proceed to give a ſhort account of theſe ſtars, and begin with thoſe of the firſt claſs.

The famous Nova of 1572 in Caſſiopea.

Several aſtronomers are of opinion, that it has a periodical return, which Keill and others have conjectured to happen every 150 years. This is alſo my opinion; and I cannot think its not being noticed at the completion of every term a material objection, ſince perhaps, as with moſt of the variables, it may at different periods have different degrees of luſtre, ſo as ſometimes to increaſe only to the ninth magnitude; and if this be the caſe, its period is probably much ſhorter. This induced me, in September 1782, to take a plan of the ſmalleſt ſtars near its place, and which I have examined often ſince, but found no alteration.

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o Ceti.

Since the end of 1782 I have obſerved very exactly the decreaſe of brightneſs of this ſtar; but never have ſeen it of above the 6th magnitude. Oct. 29, 1782, it was of the 7th magnitude, and gradually decreaſed till Dec. 30, it being then of the 8 . 9th magnitude.

1783, Feb. 16, certainly leſs than the 9th magnitude.

1783, Auguſt 25, of the 6th magnitude, and gradually decreaſed until Dec. 14, being then of the 10th magnitude, and equal to the little ſtar cloſe to it.

1784, Jan. 11, I thought it by intervals ſtill leſs than the ſame little ſtar.

1784, Sept. 12, it was of the 7 . 8th magnitude, and gradually decreaſed until Dec. 9, and then was of the 9th magnitude, and rather brighter than the little ſtar.

As a matter of curioſity, I have deduced its period from the times when it was equal to a certain ſtar in the courſe of its decreaſe; the reſults were 320—337 and 328 days; but M. Cassini determined its mean period with greater exactneſs to be 334 days. Mr. Goodricke ſaw it Aug. 9, 1782, of the 2d magnitude, rather brighter than *α* and leſs than *β* Ceti. Sept. 5, it was of the 3d magnitude, being equal to *γ* Ceti.

Algol.

The period of Algol, diſcovered by Mr. Goodricke, gave us ſome new light into the nature of the fixed ſtars. Though the phænomena ſeem to attract the attention of moſt aſtronomers, ſtill there are ſome points which require further inveſtigation. Its degree of brightneſs, when at its *minimum*, is different in different periods; and alſo, I think, when at its full, it is ſometimes brighter than α Perſei, and at other times leſs. Whether theſe differences return regularly after a certain number of periods remains yet to be examined. My laſt obſervations, when it was at the middle of its *minimum*, are,

	h.	′	
1785, July 8, at	11	50	undoubtedly leſs than ρ Perſei.
—— July 31, at	9	50	equal to ρ Perſei.
—— Sept. 12, at	10	45	a remarkable obſervation; rather leſs than δ Perſei; evidently brighter than ρ; nearly of the 3d mag.

Mayer's N° 420, lately diſcovered to be variable by M. Koch.

A few years before 1782, M. Koch ſaw the N° 420 undoubtedly leſs than the N° 419 of Mayer's Catalogue.

In February 1782, he found them both exactly of the ſame brightneſs, therefore of the 7th magnitude.

From an extract of a letter I have lately ſeen, the variable was of the 9th magnitude in April, 1783, and of the 10th in April, 1784.

I have often ſeen the N° 419, but never the variable, though I have frequently looked for it with a night-glaſs, and on the 4th of April, 1785, in a 3-feet achromatic tranſit inſtrument.

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Variable in Hydra.

Maraldi, in 1704, having found that this ſtar had a periodical variation, continued to examine it for ſeveral years, and concluded its period to be about two years, though with conſiderable variations; in which he was much miſtaken, as will appear from the following reſults, which ſhew that its period in all probability is tolerably regular, and only of 494 days.

Dates when it was at the middle of its greateſt brightneſs, eſtimated from Maraldi's obſervations.

1704, March 14, he ſaw it nearly of the ſame magnitude from the beginning of March until the beginning of April; it then decreaſed.

1705, . . . he ſaw it very faint in November, 1705, and found it decreaſing: this obſervation is too imperfect.

1708, May 22, accurately determined; its increaſe and decreaſe being well obſerved.

1709, Nov. 10, :: doubtful, its decreaſe only being obſerved.

1712, May 1, :: ditto, ditto, ditto.

1784, Jan. 26, by me, very accurately, its increaſe and decreaſe being obſerved. See the Obſervations that conclude this paper.

1785, May 27, ditto, ditto, ditto.

The four greateſt intervals of Maraldi's Obſervations give for ſingle periods in days thus 495—517—480 and 510, the mean being 500½, which is tolerably exact conſidering how doubtful the obſervations of 1709 and 1712 are. My two make it 487 days; but as the interval is only a ſingle period, it may err 10 days; I therefore ſhall take a mean between the reſults, which is 494, and proceed on to the following compariſons of MARALDI's two beſt obſervations with mine.

1708, May 22, 1784, Jan. 26,	interval of 56 periods, each of 493⅔ days.
1708, May 22, 1785, May 27,	interval of 57 periods, each of 493½ days.
1704, Mar. 14, 1784, Jan. 26,	interval of 59 periods, each of 494½ days.
1704, Mar. 14, 1785, May 27,	interval of 60 periods, each of 494⅓ days.
A ſingle period, on a mean,	494 days.

If MARALDI's obſervations of 1704 and 1708 are exact to a month, and there is no reaſon to believe otherwiſe, the period at that time ſeems to have been a few days longer than it is at preſent, and therefore the one here deduced may be eſteemed as the mean period.

Particulars of the changes it undergoes.

1. When at its full brightneſs it is of the 4th magnitude, and has no perceptible change for about a fortnight.

2. It is about ſix months in increaſing from the 10th magnitude, and returning to the ſame.

3. Therefore it may be conſidered as inviſible alſo during ſix months.

4. It is conſiderably quicker in increaſing than in decreaſing, perhaps by half.

Though when at its full it may always be ſtiled of the 4th magnitude, it does not conſtantly attain exactly the ſame degree of brightneſs, but the differences are very ſmall, as ſhewn below.

1704, brighter than ψ.

1708, brighter than ψ.

1784, much brighter than ψ, being nearly between ψ and γ Hydræ.

1785, rather brighter than ψ.

Its mean right aſcenſion, computed from my obſervations, and reduced to Jan. 1, 1784, is

° ′ ″

199 29 30 from 4 obſervations, compared to ζ ♍, made between March and May 1784.

199 29 21 from 2 ditto, compared to MAYER's 538, made in May, 1784.

199 29 20 from 5 ditto, compared to γ Hydræ, made between March and May, 1784.

199 29 24 — mean right aſcenſion for Jan. 1, 1784, on a mean.

HEVELIUS's 30th Hydræ is the above ſtar; he marks it of the 6th magnitude; I find it in no other Catalogue.

The famous Nova of 1604, in Serpentarius.

A full account of this ſtar is given by KEPLER, and it ſeems to have had a ſimilar appearance to the Nova in Caſſiopea; therefore the reflections delivered there need not be again repeated. In July, 1782, I took a plan of the ſmalleſt ſtars near its place, which was examined every year ſince, but no alteration was perceived.

β Lyræ.

Mr. GOODRICKE diſcovered the variation and period of this ſtar, and hopes ſoon to ſettle its different phaſes with more exactneſs; I ſhall therefore not enter into any detail, being certain it cannot be in better hands. In his laſt account he mentions having firſt ſuſpected the period to be only of ſix days nine hours; ſuch has always been my opinion, and which material point will probably be more ſatisfactorily determined in his next publication.

Nova near the Swan's Head of 1670.

This ſtar was firſt ſeen in December 1669 by Don ANTHELME; it ſoon became of the 3d magnitude, and diſappeared in 1672, after having undergone ſeveral variations. I have conſtantly looked for it ſince November, 1781, without ſucceſs; had it increaſed to only the 10th or 11th magnitude, I ſhould have perceived it, having taken an exact plan of all the ſurrounding ſtars.

η Antinoi.

The variation and period of this ſtar I diſcovered laſt year, and had the honour of communicating an account of it to the Society: as at preſent a long interval is elapſed ſince my firſt obſervations, and that lately I have noted ſome of its phaſes with exactneſs, I ſhall compare them to thoſe obſerved in 1784, which of courſe will give reſults more ſatisfactory. The period, as ſettled in my former paper, is 7 d. 4 h. 38′; but for reaſons there alledged, it muſt be much leſs preciſe than the following.

1785, July 18, at 9 h.
Sept. 6, at 18
Sept. 27, at 22
} times when η Antinoi was between its leaſt and greateſt brightneſs.

These

Theſe being compared to ſimilar obſervations of Sept. 12 and 19, 1784, make the length of a ſingle period thus:

D.	H.	M.
7	4	12
7	4	19
7	4	17
7	4	22
7	4	7
7	4	12
7	4	15 on a mean.

I ſee no reaſon to alter materially the other points; but believe them more exact thus:

40 hours at its greateſt brightneſs.
66 —— in decreaſing.
30 —— at its leaſt.
36 —— in increaſing.

It alſo, in every period, ſeems to attain the ſame degree of brightneſs when at its full, and to be equally decreaſed.

Variable in the Swan's Neck.

During theſe three years I have obſerved this ſtar with particular attention, as may be ſeen by the obſervations that conclude this Paper, and determined the middle time of its greateſt brightneſs very exactly, thus:

1783, July 9, of the 6 . 7th magnitude.
1784, Aug. 4, of the 5 . 6th ———
1785, Sept. 1, of the 6th ———

The ſecond of theſe, being compared to that of Nov. 20, 1687, made by KIRCH, gives 406 days exactly for one period, the interval between them being 35322 days, and divided by 87 periods. I make the diviſor 87, in order to get a reſult neareſt to that ſettled by MARALDI and CASSINI of 405, and by M. LE GENTIL of 405,3 days. We cannot ſuppoſe that theſe great aſtronomers have made any miſtake; and on the other hand, it ſeems hardly poſſible, that the mean of my obſervations alone, which makes the period 392 days, can err 14; but perhaps its period is irregular; to determine which ſeveral intervals of 15 years ought to be taken, and I am much inclined to believe, that it will be found of only 396 days 21 hours.

Particulars of the changes it undergoes.

1. When at its full brightneſs it has no perceptible change for about a fortnight.

2. It is about 3½ months in increaſing from the 11th magnitude to its full brightneſs, and the ſame in decreaſing.

3. Therefore it may be conſidered as inviſible during ſix months.

4. It does not attain the ſame degreee of brightneſs at every period, being ſometimes of the 5th, and other times of the 7th magnitude.

Its mean right aſcenſion, computed from my obſervations, and reduced to Aug. 1, 1783, is

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295	33	46	from 2 obſerv. compared to γ Cygni, made in July and Auguſt, 1783.
295	33	45	from 2 ditto, compared to γ Lyræ, ditto.
295	33	45	from 1 ditto, compared to α Lyræ, made in Auguſt, 1783.
295	33	55	from 3 ditto, compared to β Lyræ, made in July and Auguſt, 1783.
295	33	52	from 2 ditto, compared to β Cygni, made in Auguſt 1783.
295	33	48½	mean right aſcenſion for Auguſt 1, 1783, on a mean.

Variable in the Swan's Breaſt.

This ſtar was firſt ſeen by G. JANSONIUS in 1600, and afterwards frequently obſerved by different aſtronomers, but with intervals of ten or more years, which is probably the reaſon why no regularity in its changes has yet been deduced. I have examined minutely the obſervations made in the laſt century, and ſhall venture to give the following reſults.

1. Continues at its full brightneſs for about five years.
2. Decreaſes rapidly during two years.
3. Inviſible to the naked eye for four years.
4. Increaſes ſlowly during ſeven years.
5. All theſe changes, or its period, are completed in 18 years.
6. It was at its *minimum* at the end of the year 1663.

It does not always increaſe to the ſame degree of brightneſs, being ſometimes of the 3d, and at other times only of the 6th magnitude. I am intirely ignorant whether it is ſubject to the ſame changes ſince this century, having not met with any ſeries of obſervations on it; but if the above conjectures are exact, it will be at its *minimum* in a very few years. Since November, 1781, I have conſtantly ſeen it of the 6th magnitude, being rather leſs than N° 28, 29, and *m*, and rather brighter than

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36 and 40 Cygni. Sometimes I ſuſpect it has rather decreaſed within theſe two laſt years, though in a very ſmall degree.

Its mean right aſcenſion, computed from my obſervations, and reduced to Sept. 1, 1782, is

302 26 43 from 3 obſervations, compared to γ Cygni, made in October, 1781.
302 26 46 from 3 ditto, compared to γ Cygni, made in Auguſt, 1782.
302 26 52 from 1 ditto, compared to β Aquilæ, made ditto.
302 26 46 from 1 ditto, compared to α Cygni, made ditto.
302 26 39 from 1 ditto, compared to δ Andromedæ, made in October 1781.

302 26 45 mean right aſcenſion for Sept. 1, 1782, on a mean.

Flamsteed has this ſtar in his Catalogue; but, I believe, obſerved it only once.

δ Cephei.

This is the laſt variable ſtar diſcovered, and again by Mr. **Goodricke**. Its changes are very difficult to be ſeen, unleſs examined when at its *minimum* and full brightneſs. I have lately made ſome good obſervations on it thus:

1785, Aug. 30, at 14 h. leſs than ε Cephei.
31, at 9 h. equal if not brighter than ζ Cephei.
Sept. 15, at 12½ h. leſs than ε Cephei.
16, at 8 h. between ε and ζ Cephei.
— at 11 h. increaſed, but not as bright as ζ.
17, at 11 h. rather brighter than ζ.
26, at 11½ h. equal or leſs than ε.
27, at 8 h. evidently brighter than ζ.

Therefore it was between its leaſt and greateſt brightneſs Auguſt 31, at noon, and Sept. 26, at 21 h.: theſe being compared to *my* firſt obſervations, when alſo between its leaſt and greateſt brightneſs on Nov. 20, at 3 h. and Nov. 30, at 15 h.

1784,

1784, give the following reſults, the mean of which corroborates that deduced by Mr. GOODRICKE of 5 d. 8 h. 37′½.

	D.	H.	M.
	5	8	35
	5	8	41
	5	8	33
	5	8	39
Length of a ſingle period	5	8	37 on a mean.

δ Cephei concludes the ſtars of the firſt claſs; thoſe that follow are of the ſecond.

HEVELIUS's 6th Caſſiopeæ.

In 1782 I firſt perceived that this ſtar was miſſing; nor could I find it in 1783 and 1784.

46th or ξ Andromedæ.

This ſtar is ſaid to have diminiſhed in brightneſs. In 1784 and 1785 I found it, by very exact obſervations, leſs than *υ*, equal to *ω**, and brighter than *d* and *χ*; yet I muſt mention that it is marked in my journal as being ſometimes brighter, and at other times leſs than *ω**; but ſtill I am not convinced, that it varies in brightneſs. FLAMSTEED, in his Catalogue, annexes no character to his 46th Andromedæ; but in vol. II. of his Hiſt. Cœleſt. p. 135. and 138. he marks it ξ.

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* I ſuſpect an error in this character, but cannot be certain. H. E.

FLAMSTEED's 50, 52, τ Andromedæ, and HEVELIUS's 41 Andromedæ.

As the poſition and characters of theſe ſtars differ conſiderably in different Catalogues, and that ſome of them are mentioned by CASSINI to have diſappeared and re-appeared, I ſhall give their brightneſs as obſerved in 1783, 1784, and 1785.

FLAMSTEED's 50th of the 4 . 5th magnitude, and equal, if not rather leſs than φ Andromedæ.
——— τ of the 5th magnitude, and equal to 46 and 48 Andromedæ.
——— 49 }
——— 52 } of the 5 . 6th magnitude, and are of the ſame brightneſs.
HEVELIUS's 41 }

A ſtar between FLAMSTEED's 52 and HEVELIUS's 41 is of the 6th magnitude, or rather leſs. I could not ſee TYCHO's 19th Andromedæ; but I take this ſtar to be the ſame as HEVELIUS's 41 Andromedæ.

TYCHO's 20th Ceti.

This muſt be the ſtar which HEVELIUS ſaid had diſappeared, being TYCHO's ſecond in the Whale's belly. There can hardly be any doubt but that it is the χ, miſplaced by TYCHO. This χ is of the 4 . 5th magnitude, and of the ſame brightneſs as the three ψ Aquarii.

FLAMSTEED's 55th Andromedæ, marked Neb. in his Catalogue.

It is mentioned in the lateſt Catalogues of Nebulæ that this nebula could not be found. FLAMSTEED, who, I believe, only obſerved it once, *viz.* Oct. 17, 1691, does not mark it nebulous; nor does it appear to me ſuch, but as a ſtar of the 6th magnitude. There are a few ſmall ſtars near it, which to the naked eye, when the air is very clear, make it appear nebulous, which probably is the reaſon why FLAMSTEED marked it thus in his Catalogue.

σ or PTOL. and UL. BEIGH's 17th Eridani.

FLAMSTEED ſays, he could not ſee this ſtar in 1691 and 1692. In 1782, 1783, and 1784, I obſerved one of the 7th magnitude in that place; the relative brightneſs of which appeared always the ſame, *viz.* leſs than two little ſtars near and below η Eridani.

FLAMSTEED's 41 Tauri.

This ſtar was thought by CASSINI to be a new one or variable. I ſee little or no reaſon to be of that opinion; that it is not new is evident, ſince it is UL. BEIGH's 26th and TYCHO's 43d. In 1784 and 1785 I found it of the 5th magnitude, being equal to φ, and brighter than ψ, P, and χ Tauri.

Star about 2°¼ North of 53d Eridani, and 47 Eridani.

The firſt of theſe ſtars CASSINI thought a new one, and that it was not viſible in 1664. In 1784 I found it was leſs than ω, and *d*, brighter than A, and ſeemed equal to ψ Eridani.

CASSINI mentions another ſtar thereabouts, which he alſo eſteemed a new one: this is probably FLAMSTEED's 47th. In 1784 it appeared rather leſs than 46th.

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γ Canis Majoris.

MARALDI could not ſee this ſtar in 1670; but in 1692 and 1693 it appeared of the fourth magnitude. I have very frequently noticed it ſince 1782, but perceived not the leaſt variation, being conſtantly of the 4th magnitude, very little brighter than *θ*, and decidedly brighter than *ι*.

α β Geminorum.

If either of theſe ſtars have changed in brightneſs, it is probably the *β*. In 1783, 1784, 1785, the *β* was undoubtedly brighter than *α*.

ξ Leonis.

MONTANARI ſays, this ſtar was hardly viſible in 1693. I found it conſtantly in 1783, 1784, and 1785, of the ſame brightneſs, being of the 5th magnitude; leſs than A, *π*, and, if any difference, rather brighter than *h* and *ω* Leonis. TYCHO, FLAMSTEED, MAYER, BRADLEY, &c. mark it of the 4th magnitude.

ψ Leonis.

This ſtar is ſaid to have diſappeared before the year 1667. It is now, and has ever been ſince 1783, of the 5 . 6th magnitude, being leſs than *ω*, and brighter than *i*, FLAMSTEED's 46th.

25th Leonis.

In 1783 I firſt perceived this ſtar was miſſing; nor was it viſible in 1784 and 1785, even with the tranſit-inſtrument.

BAYER's *i* Leonis, or TYCHO's 16 Leonis.

It was not viſible in 1709, nor could I ſee it in 1785. This is a different ſtar from the *i* Leonis of the other Catalogues, though TYCHO's *deſcription* of its place is the ſame.

δ Urſæ Majoris.

This ſtar is ſuſpected to change in brightneſs (ſee LONG's Aſtronomy), on account of its being marked by TYCHO, Prince of HESSE, &c. of the 2d magnitude; while HEVELIUS, BRADLEY, and others, have it of the 3d. At preſent, and for theſe three years paſt, it appears as a bright 4th magnitude, being rather leſs than ι, equal to α, and rather brighter than κ Draconis.

n Virginis.

This ſtar is ſuppoſed to be variable, becauſe FLAMSTEED, on the 27th of January, 1680, ſays he could not ſee it. He obſerved it May 12, 1677, and ſome years afterwards, ſince it is in his Catalogue. I examined it frequently in 1784 and 1785, without perceiving the leaſt change, being of the 6th magnitude, leſs than *c*, and *rather* brighter than a ſtar three degrees lower in a right line with *c* and *n* Virginis.

Bayer's ſtar of 6th magnitude, 1° South of *g* Virginis.

This ſtar is not in any of the nine Catalogues that I have. Maraldi looked for it in vain; and in May, 1785, I could not ſee the leaſt appearance of it. It certainly was not of the 8th magnitude.

In the northern thigh of Virgo.

This ſtar, which is marked by Ricciolus of the 6th magnitude, could not be ſeen by Maraldi in 1709; nor was it of the 9th magnitude, if at all viſible, in 1785.

91 or 92. Virginis.

In 1785 I found that one of theſe ſtars was miſſing, and which ſeems to be the 91; the remaining one is of the 6 . 7th magnitude.

α Draconis.

I am of Mr. Herschel's opinion, that it is highly probable this ſtar is variable. Bradley, Flamsteed, &c. mark it of the 2d magnitude; at preſent it is only of a bright 4th. I have frequently examined it ſince October, 1782, without perceiving the leaſt change, being conſtantly rather leſs than ι Draconis, equal to δ Urſæ Majoris, and rather brighter than κ Draconis.

Bayer's ſtar in the weſt ſcales of Libra.

Maraldi ſays he could not ſee this ſtar; nor could I in 1784 and 1785. With a night-glaſs may be ſeen thereabouts ſome ſmall ſtars of about the 8th magnitude, none of which are near as bright as the 2d ν Libræ.

PTOL. and UL. BEIGH's N° 6 of the unformed in Libra.

In examining different Catalogues I do not find this ſtar in any other than the above, though it is marked of the 4th magnitude. If PTOLEMY had not the κ it might be thought to be that. In 1785 I frequently obſerved a ſtar of the 7th magnitude very near its place, which appeared rather leſs than FLAMSTEED's 41. FLAMSTEED has not this little ſtar in his Catalogue; but he obſerved it May 9, 1681.

κ Libræ.

This ſtar is thought to be variable. I am not of that opinion; though certainly it is rather ſingular that HEVELIUS, whoſe attention was directed to this part of the heavens, to find TYCHO's 11th, did not obſerve the κ; and the more ſo, as he has noticed two much leſſer ſtars not far from it. During theſe three years I have found the κ conſtantly of the 5th magnitude, being leſs than ψ or θ, equal to λ, and brighter than η.

TYCHO's 11th Libræ.

HEVELIUS ſays he could not find a ſtar of the 4th magnitude in Libra noticed by TYCHO. This muſt be TYCHO's 11th, ſince he has all the others. It was not viſible in 1783, 1784, and 1785, nor probably ever exiſted; for it is, I think, evident, that this 11th is no other than the κ, with an error of two degrees in longitude.

53 Serpentis.

In 1784 I perceived that this ſtar was miſſing; nor was it viſible in 1785 with a night-glaſs.

A ſtar marked by BAYER near ε Urſæ Minoris.

CASSINI could not ſee this ſtar. In 1782 I took, with a night-glaſs, a plan of all the ſtars near its place, and near the ε, none of which were brighter than the 7 . 8th magnitude. I have ſince re-examined the plan, but found no alteration.

The ρ or PTOL. and UL. BEIGH's 14th Ophiuchi or FLAMSTEED's 36th.

I have no doubt but that this is the ſtar which is ſaid to have diſappeared before 1695. It is alſo evident, by what HEVELIUS ſays in his Catalogue on the θ and B, that the ρ was not ſeen by him. In 1784 and 1785 I found it of the 4 . 5th magnitude, much brighter than 39, alſo rather brighter than 51 and 58, and leſs than 44. On the 30th of June, 1783, I have marked it in my journal equal to 39, and leſs than 51 and 58; but as the obſervation was not repeated, I am far from being certain it has undergone any change, particularly as this ſtar has a ſouthern declination of 26°, and therefore great attention muſt be given to the ſtate of the atmoſphere.

PTOL. 13th and 18th Ophiuchi, 4th magnitude.

If there is no error in the Catalogue, theſe two ſtars have diſappeared; but I am confident that PTOLEMY's 13th is FLAMSTEED's 40th, and that PTOLEMY's 18th ought to be marked with a north latitude inſtead of ſouth, which would make it agree nearly with FLAMSTEED's 58th.

σ Sagittarii.

Mr. HERSCHEL, with great reaſon, has placed this ſtar among thoſe which probably have changed their magnitudes. I had long ſince remarked the ſingular diſagreement in all the Catalogues, which induced me to obſerve it frequently, particularly in 1783, 1784, and 1785, when it appeared of the 2.3d magnitude, and brighter than π Sagittarii.

θ Serpentis.

MONTANARI ſays he ſaw this ſtar of the 5th magnitude, and that the next year it grew bigger. I examined it frequently in 1783, 1784, and 1785, and found it always leſs than δ Aquilæ, equal to β Aquilæ, and P Ophiuchi; 4th magnitude.

TYCHO's 27th Capricorni.

This ſtar was not viſible in HEVELIUS's time; nor could I ſee it 1778, 1782, 1784, with the tranſit-inſtrument.

Tycho's 22d Andromedæ and *o* Andromedæ.

Cassini remarked, that the ſtar placed by Tycho at the end of the chain of Andromeda as of the 4th magnitude, was grown ſo ſmall that it could ſcarcely be ſeen. This is Tycho's N° 22, the longitude and latitude of which places it near the two *π* Cygni, and where no ſtar was viſible in 1784 and 1785.

As poſſibly, by Tycho's deſcription, Cassini took the 22d for the *o* Andromedæ, I have alſo examined this ſtar, and in 1783, 1784, and 1785, found its relative brightneſs thus: leſs than *α* Cephei; equal to *ζ* Caſſiopeæ, though, if any difference, rather brighter; and brighter than *λ*, *κ*, or *ι* Andromedæ.

Tycho's 19th Aquarii.

This is the ſtar that Hevelius ſays was miſſing, and that Flamsteed could not ſee with his naked eye Nov. 18, 1679; nor could I ſee the leaſt appearance of it in 1782. I am convinced it is the ſame ſtar as Flamsteed's 56th, marked *f* by Bayer, from which it is only 1°¼. Flamsteed's 53d, marked *f* in Ptolemy's Catalogue, is a different ſtar.

La Caille's 483 Aquarii.

I firſt diſcovered that this ſtar was miſſing in 1778. It was not viſible in 1783, 1784.

There are a few other ſtars ſuſpected by the ancient aſtronomers to have been new or altered a little in brightneſs, which I have omitted, not ſeeing any reaſon to think them ſo; and ſome that are certainly variable, but cannot be obſerved in theſe latitudes. I have alſo, contrary to my firſt intention, added ſeveral which are not mentioned by them; ſuch are thoſe that I lately diſcovered to be miſſing.

Perhaps many perſons would place in the firſt claſs ſeveral ſtars which I have put in the ſecond, relying on the poſitive aſſertions we have of their having diſappeared, diminiſhed, or being new; for my part, I am confident that moſt of theſe ſuppoſed changes may be attributed to miſtakes; and in general for thoſe that are ſaid to be loſt, an attentive compariſon of different Maps and Catalogues will point out the error; and thus I have ventured to give my opinion of TYCHO's 20th Ceti, 11th Libræ, and 19th Aquarii, &c. Since FLAMSTEED's Catalogue has been more particularly inveſtigated, the number of theſe ſuppoſed loſt ſtars is conſiderably increaſed; but if the ſecond volume of his Hiſt. Cœleſt. is alſo examined, many errors will be detected; among which it will appear very unaccountable, that the 71ſt, 80th, and 81ſt of Hercules, which were diſcovered to be miſſing by Mr. HERSCHEL, are not in FLAMSTEED's obſervations under the name of Hercules, though I looked for them with particular attention, and find the 70th. The 19th Perſei, which Mr. HERSCHEL alſo could not ſee, was obſerved but once by FLAMSTEED, *viz.* Jan. 16, 1693, and in all probability is the τ with the time of its tranſit erroneouſly ſet down. The τ was obſerved on Jan. 17, 1693, and Jan. 18, 1694. Beſides theſe Mr. GOODRICKE has found ſeveral other errors ſtill more evident. I ſcarcely need add, that theſe corrections do not in the leaſt intimate any miſtake or diminiſh the merit of thoſe that firſt point them out, but fall entirely on the ancient catalogues and obſervations.

Mr. HERSCHEL, in ſelecting ſeveral ſtars which poſſibly may be reckoned new ones, very judiciouſly gives us plauſible reaſons not to lay great ſtreſs on their being ſo; and the following remark only confirms what he there ſuggeſts; for that ſtar of the 5th magnitude following τ Perſei, mentioned by him, and which with great reaſon might be eſteemed a new one, is in all probability the ſame as one obſerved by FLAMSTEED, Jan. 18, 1694, though not inſerted in his Catalogue.

With regard to thoſe ſtars which are ſaid to have diminiſhed or increaſed, as thoſe in Andromeda, Leo, &c. they are, in my opinion, far from being confirmed as variable. I know, from repeated experience, that even more than a ſingle obſervation, if not particulariſed and compared with neighbouring ſtars, is very little to be depended on; different ſtates of the weather, thin ſtreaks of clouds, have ſeveral times made me err a whole magnitude in the brightneſs of a ſtar.

Whether theſe apparent changes in the ſtars proceed intirely from themſelves; or whether they are effected by any foreign power that may in part occaſion ſome of their particular appearances and irregularities, we have not ſufficient *data* to determine: but whatever are the cauſes, a diviſion of the different phænomena ſeems to be the moſt probable means of forwarding any conjecture that hereafter may be formed; I ſhall therefore divide the firſt claſs into three orders.

The firſt contains thoſe that are periodical with long intervals; and ſuch I reckon o Ceti, that in Hydra, that in the Swan's breaſt and neck, and alſo MAYER's N° 420.

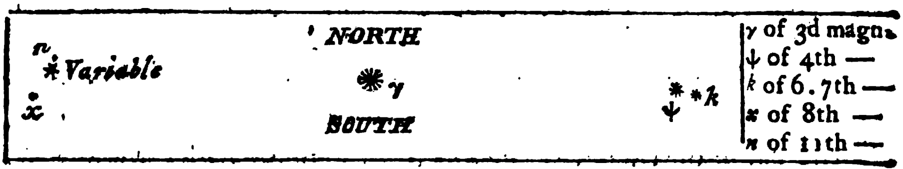
For the ſecond order I ſhall mention only three, though others might be added; but the accounts of them are ſo unſatisfactorily recorded, and their places ſo little known, that I prefer ſelecting only that in Caſſiopea of 1572, that in Serpentarius of 1604, and that near the Swan's head. The phænomena of theſe certainly bear a great reſemblance to the firſt: ſtill their ſudden appearance, and no certainty of a period, or at leaſt infinitely longer, are, I think, ſufficient reaſons to ſeparate them.

Laſtly, Algol, η Antinoi, β Lyræ, and δ Cephei, are ſo ſimilar to each other, and ſo different from the above, that there can be but little or no heſitation in diſtinguiſhing them; alſo the cauſe of their changes ſeem in general to be attributed to ſpots, and a rotation on their axis. This property of the fixed ſtars, though often ſuſpected, was far from being evident till within theſe two years; and we are not only indebted to Mr. GOODRICKE for the diſcovery of the firſt, but alſo for three of the only four known.

Further may be added, that all thoſe of the firſt order (MAYER's 420 being yet ſo little known remains doubtful) attain in different periods different degrees of brightneſs when at their full; alſo the progreſſive increaſe of brightneſs of that in Hydra, that in the Swan's breaſt, of *a* Ceti, β Lyræ, η Antinoi, and δ Cephei, is not ſimilar to their decreaſe. This peculiarity with regard to Algol is yet uncertain, owing to the rapidneſs of its changes, ſo that there is only one that ſeems to have theſe points uniform, *viz* the variable in the Swan's neck.

I ſhall now conclude with the obſervations from which ſome reſults, given in this Paper, have been deduced; they are here collected together, in order to avoid confuſion.

Obſervations on the variable in Hydra.



1783, Dec. 11. A.M.	much leſs than ψ, and rather leſs than k.
24. A.M.	equal, if not leſs than, ψ; of the ſame colour as ψ.
1784, Jan. 4. A.M.	brighter than ψ; of about ¼ of the difference between ψ and γ; of a more copper colour than ψ.
9. A.M. 10. A.M.	if any difference brighter.
25. A.M.	with the naked eye, it appeared in brightneſs nearly between ψ and γ; and of a more copper colour than either ψ or γ.
Feb. 1. 2.	of the ſame brightneſs, but ſeemed of a more copper colour.
23.	between ψ and k; air not clear.
29. March 10.	between ψ and k, but nearer the brightneſs of ψ.
April 11. 12. 19. May 2.	rather brighter than k.
9.	rather leſs than k, and brighter than x.
1784, Dec. 1. A.M.	did not ſee the variable; ſtrong moon-light.
9. A.M.	ditto ditto.
22. A.M.	ditto ditto.
1785, Mar. 4. or 11.	ditto ditto.
April 17.	ſaw the x, but not the variable.
May 4.	viſible to the naked eye; leſs than ψ, and much brighter than k; of a more copper colour than ψ.
7. 11.	rather leſs than ψ.

May

1785, May 14. equal to ψ; air not very clear, and moon-light.
15. equal to ψ; air clear; moon-light.
19. undoubtedly brighter than ψ; little hazy; moon near.
22. 27. } rather brighter than ψ; moon-light ſtrong.
June 10. rather brighter than ψ, though I think decreaſed.
12. equal to ψ: but am not ſure if the ſky was quite clear.
13. rather leſs bright than ψ; air clear.

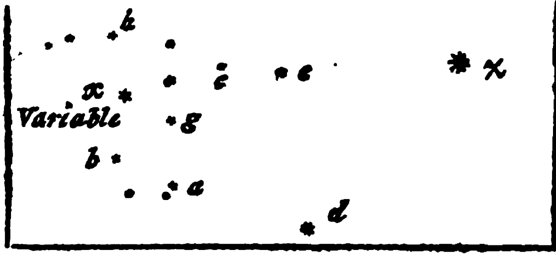
Mr. Goodricke alſo frequently obſerved the variable, and his obſervations agree with the above.

Obſervations on η Antinoi.

h.
1785, May 20. at 12½ equal or leſs than ι Antinoi.
21. — 12½ equal to ι, evidently leſs than β; moon-light.
22. — 12½ equal if not brighter than β Aquilæ; moon.
— — 12¾ thought it brighter than β; air clear.
June 10. — 12½ rather brighter than ι Antinoi.
12. — 11¾ rather brighter than ι, leſs than β.
13. — 12½ a little brighter than β, much leſs than δ.
July 15. — 12½ leſs than β, brighter than ι.
17. — 11 leſs than ι, brighter than μ.
18. — 11 between ι and β, I think rather nearer β.
19. — 10½ 11½ } between β and δ, rather nearer β; air clear.
20. — 11 rather brighter than β, certainly equal.
Aug. 30. — 9½ rather brighter than β, at leaſt equal to it.
31. — 9 much brighter than β.
Sept. 6. — 9 leſs than ι; a ſingle view of it, not very ſatisfactory.
7. — 9½ much brighter than β.
26. — 9¼ 10½ } much leſs than β or ι, brighter than μ.
27. — 8 10½ } much leſs than β, leſs than ι, brighter than μ.
28. — 9½ *rather* brighter than β.
— — 11 increaſed a little in brightneſs.

The brightneſs of the ſtars to which *η* Antinoi is compared are given in the Philoſophical Tranſactions, vol. LXXV. part I.

Obſervations on the variable in Cygnus's Neck.



χ of 6th mag.
d of 7th —
e of 7th —
a of 8th —
b of 8th —
c of 8 . 9 —
h of 9th —

As KIRCH's plan (ſee Phil. Tranſ. abridged by JONES) is much the ſame as this, I have annexed the ſame letters to the ſtars.

Dates.	Mag.	
1783		
Mar. 28	0	if viſible, not of the 8th magnitude.
June 9	7	rather brighter than *d* or *e*.
11	7	rather increaſed.
23	6 . 7	leſs than *χ*.
30	6 . 7	rather increaſed.
July 15	6 . 7	of the ſame brightneſs as on the 30th of June.
27	7	a little brighter than *d* or *e*.
Aug. 7	7	rather brighter than *d*; decreaſed.
8	7	equal to *d*.
16	7	leſs than *d*, brighter than *e*.
25	7 . 8	leſs than *e*.
Sept. 4	7 . 8	think brighter than *a*.
12	8	equal to *a*.
22	8 . 9	much leſs than *a*, equal to *c*, and brighter than *h*.
24	9	equal to *h*.
Oct. 14	9 . 10	leſs than *h*.
24	10 . 11	ſeen with the greateſt difficulty; leſs than any ſtars of the plan; am doubtful if I ſaw it by intervals.
27		
1784	0	I looked conſtantly for the variable between October and April, but could not ſee it.
April 23	10 . 11	leſs than any ſtars of the plan.
24	10 . 11	ditto.
May 9	9 . 10	rather leſs than *h*.

1784

Dates.		Mag.	
1784			
May	21	8 . 9	rather brighter than *b*, but not ſo bright as *b*.
June	17	8	equal to *a*, brighter than *b*.
July	21	6	equal to χ with the naked eye.
	22	6	if any difference the χ was the brighteſt.
	27	6	rather brighter than χ.
Aug.	1	5 . 6	undoubtedly brighter than χ.
	10	6	think it decreaſed, but ſtill rather brighter than χ.
	13 19	6	rather leſs than χ.
Sept.	2	6 . 7	much decreaſed, leſs than χ, but brighter than *d*.
	12	6 . 7	ſtill brighter than *d*.
	19	7	if any difference brighter than *d*.
	20	7	rather leſs than *d*.
Oct.	5	7 . 8	leſs than *e*, about equal to *a*.
	16	7 . 8	rather brighter than *a*.
Nov.	11	9	leſs than *b*, about equal to *f*.
	17	10	rather leſs than *f*.
1785			
May	9	0	not viſible.
June	21	9 . 10	leſs than *b*.
July	23	8	rather brighter than *a*.
Aug.	13 15	7	a little brighter than *d* and *e*, *much leſs* than χ; am not ſure I could ſee it with the naked eye.
	27 28	6 . 7	not ſo bright as χ, much brighter than *d*; I could ſee it with the naked eye, but the χ more diſtinctly.
	30	6	not quite ſo bright as χ; naked eye.
Sept.	2 7	6	ditto ditto.
	11 12	6 . 7	very difficult to ſee with the naked eye, decreaſed in brightneſs; air clear.
	26 27	7	though the air was remarkably clear, it was with the utmoſt difficulty I could ſometimes ſee it with the naked eye.

⁂

X. *An Account of a Subſidence of the Ground near* Folkſtone, *on the Coaſt of* Kent. *In a Letter from the Rev.* John Lyon, *M. A. to* Edward King, *Eſq. F. R. S. and A. S. Communicated by Mr.* King *in a Letter to* Charles Blagden, *M. D. Sec. R. S.; with Remarks.*

Read February 16, 1786.

TO DR. BLAGDEN, Sec. R. S.

DEAR SIR, Mansfield-Street, Dec. 22, 1785.

HAVING always thought the account given in the Philoſophical Tranſactions, by Mr. SACKETTE *, about the beginning of this century, concerning the motion of the Cliffs, and of the adjacent ground, near Folkſtone in Kent, a very curious one, and deſerving of much attention; both becauſe of the many poſitive atteſtations there were of *ancient* men with regard to it (who were both mariners, and uſed to obſervation); and becauſe of the ſingular conſequences that would follow, from the aſcertaining of ſuch a fact, in a philoſophical light; I have conſtantly, whenever I had any opportunity, made repeated enquiries concerning the matter, of ſuch perſons as I thought likely to be able to afford me any ſatisfactory information.

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* See Phil. Tranſ. vol. XXIX. N° 349. or JONES's Abridgment, vol. IV. part II. p. 248.

Amongſt the reſt, I mentioned it laſt ſummer to my worthy and very curious friend Mr. BOYS, of Sandwich; who ſeemed ſurpriſed at the narration, and had never before heard of any ſuch phænomenon. However, in leſs than a fortnight after our converſation, I was agreeably ſurpriſed by receiving a letter (dated 24th Sept. 1785), in which he ſaid, *I am ſorry your health will not permit you to make the tour you at firſt propoſed; eſpecially as ſomething* VERY CURIOUS *has happened within theſe few days at Folkſtone. Part of the cliff, to the weſtward of the town, a little way from the church, has ſunk, and continues ſinking into the earth; raiſing the ground, about the ſinking part, in a very extraordinary manner. This correſponds with what you ſaid to me on the ſubject of Stutfall-Caſtle, &c.; and certainly deſerves your attention. If I could, by going thither, give you any ſatisfaction, I ſhall be ready and happy to obey your commands.*

Being, through illneſs, prevented from examining this curious phænomenon myſelf, I accepted this obliging offer of Mr. BOYS, and requeſted his aſſiſtance: and although he alſo was prevented from going to the ſpot himſelf, yet he applied to a friend of his, the reverend Mr. LYON, of Dover; who has made repeated viſits to the place, to obtain all the information poſſible; and has, at laſt, ſent to me a very accurate drawing, together with an explanatory letter; which I now, with great pleaſure, venture to lay before the Royal Society.

I muſt, however, at the ſame time, beg leave to obſerve, that although Mr. LYON differs from Mr. SACKETTE, in his concluſion, concerning the motion of the whole adjacent country, and controverts *that* fact; and has certainly given a more clear and ſatisfactory account of the preſent phænomenon, than Mr. SACKETTE did of that which he wiſhed to record; yet they both agree in imputing *a moſt remarkable effect* (only in different ways) to the paſſage of the ſprings, and drains of water, through the ſtratum of looſe marle, on which the whole country reſts: an effect which muſt needs produce, at different periods of time, various alterations on the ſurface; and may moſt probably have ocaſioned much greater changes in the face of the country than that made in the preſent inſtance.

Whether therefore Mr. SACKETTE was right or wrong, in his great and final concluſion, concerning the motion of the *whole coaſt*; what he records, on the teſtimony of ſo many aged perſons (in which they perſiſted with great ſeriouſneſs and on the fulleſt conſideration), does ſurely ſtill deſerve at leaſt to be born in mind, and to be attended to with much circumſpection; eſpecially on a coaſt, where perhaps *fixed points* may be attempted to be aſcertained, at ſome time or other, in order to complete the moſt accurate and moſt curious philoſophical menſurations.

And I muſt further venture to obſerve, in vindication of Mr. SACKETTE's account and concluſions, that although Tarlingham-houſe has indeed been rebuilt, ſince the time referred to by the old man who converſed with Mr. LYON; and therefore *that* old man's remark might be occaſioned merely by *that* circumſtance; yet it had not been rebuilt in Mr. SACKETTE's time; and, therefore, no ſuch circumſtance could be the occaſion of its coming *recently in view*, at that period when he wrote, in parts of the coaſt where it had not been poſſible to ſee it at ſome time before.

And as to the MOORING ROCK, *ſo particularly referred to by Mr.* SACKETTE, *being now utterly unknown*; it ought to be remembered, that Mr. SACKETTE, in his deſcription of it, ſays, *that it lies ſurrounded with great numbers of other rocks, and was on this account chiefly a noted one, becauſe at it veſſels uſe to* *be moored, while they are loading other rocks, which they take from hence, not only for our own pier-heads, but for thoſe of Dover Pier; and a very great quantity of them were ſhipped in the time of* OLIVER's *uſurpation, and carried to Dunkirk, for the ſervice of that harbour.* Conſidering, therefore, that the enormous pile of Ramſgate Pier has been built ſince that time, (which, though it be chiefly compoſed of Portland ſtone, had, I apprehend, foundations and interior parts of ruder materials) and that there have been other vaſt demands for ſtone, it is not at all unlikely, that this very *Mooring Rock*, mentioned by Mr. SACKETTE, has itſelf been carried away in like manner as the others were that uſed to ſurround it; and that this is the ſole reaſon why it is now no longer known, and totally forgotten.

I am, Sir, with much eſteem, &c.

EDWARD KING.

P. S. There is a *peculiarity* in Mr. LYON's ſketch, Tab. V. (deſigned to illuſtrate the grounds of his objections to Mr. SACKETTE's concluſions) which demands ſome explanation. After having given a ſection of the cliff and ſhore, the lines (inſtead of being continued in the ſame plane, and in the direction of the *ſame* ſection) are drawn ſo, as to be conceived as extended *on the ſurface of the country* from the eye of the obſerver at E. Without attending to this circumſtance, what he ſays is not very eaſily to be underſtood; and indeed I muſt ſtill think, that Mr. SACKETTE does not deſerve ſo much cenſure, although Mr. LYON's be undoubtedly a moſt accurate account, and moſt clear ſolution of the preſent phænomenon.

TO EDWARD KING, ESQ.

SIR, Dover, Nov. 24, 1785.

AS I have been requeſted by my friend Mr. BOYS, of Sandwich, to examine into the cauſe of the ſinking of ſome ground near the town of Folkſtone, in this neighbourhood, and to ſend you the reſults of my inquiries; I have made it my buſineſs to attend particularly to the ſubject. I have been twice to view the place. I have endeavoured to procure the beſt information, and have compared my remarks with what the reverend Mr. SACKETTE formerly ſaid upon the ſame ſubject to the Royal Society.

That you may have a clearer idea of the place where the ground is ſinking, I have annexed a drawing of it, taken from a ſmall hill near the foot of the cliff.

AA (Tab. IV.) repreſents the length of the ground, 130 feet, which is ſunk 40 feet from the top of the cliff DD.

BB, is a fiſſure, in the valley between the ſinking ground AA and the hill HH, and in which there are many ſmaller chaſms.

C, the tower of Folkſtone-Church, not far from the cliff DD.

E, part of the town of Folkſtone, as ſeen between the cliff DD and the hill HH.

F, the high chalk cliffs at a diſtance, leading towards Dover.

G, a track of paſture land, between a high range of hills and the ſea.

I, the beach, at the foot of the hill H.

KK, Rocks, ſaid to be raiſed (and I believe they are) by the ſinking of the ground AA.

As I intend, in explaining the cauſe of the ſinking of the ground AA to you, to advance an opinion of my own, and to controvert what the reverend Mr. SACKETTE formerly ſaid upon the ſubject, it may be neceſſary to explain the nature of the ſoil, as far as it is open to view, in the neighbourhood of Folkſtone.

The chalk cliffs FF, which begin at Dover, form oppoſite Folkſtone town high hills, and leaving the ſhore, there is a a large track of arable and paſture land between them and the ſea.

Part of this ground is ſhewn in the view at G, and is a kind of marle, which contains pyrites, fragments of the Cornu Ammonis, and many other foſſil bodies.

Next to the marle is a looſe ſandy ſoil (ſee the cliff DD) intermixed with a very large, hard, and coarſe kind of ſtone, in which are often found foſſil oyſter ſhells.

This ſandy ſoil reſts upon a marle, which at the cliff DD is in ſome places three or four feet above the beach, and when wet is very ſlippery. A ſtratum of this marle extends for many miles on the coaſt, and where it is not ſufficiently covered with ſand to bear any weight, it is in many places a quag, and dangerous to paſs over.

Through this track of land I have deſcribed, there are many drains of water, which may be ſupplied partly from the falling of the rains in wet ſeaſons, and partly from the ſprings iſſuing from the hills; and there is reaſon to ſuppoſe, that in a looſe ſoil theſe drains form channels in a courſe of time. At the place where the ground has ſunk before, and is now ſinking, there is a drain from the marle under the ſand; and I am of opinion, that the courſe of the water is in the ſame direction as the valley between the hill H and the ſinking of the ground AA.

That the ſinking of the ground is cauſed by the foundation being undermined (and I think by water) is evident from the appearance of the ground in the valley. The ſoil is full of fiſſures, and reſembles an arch, which is ſunk down, and has left the two abutments, the hill H and the cliff DD, ſtanding.

As the hill H more than counter-balances the preſſure of the ſinking ground upon the ſtratum of wet marle, the conſequence is, that the rocks KK, at ſome yards diſtance, being only thinly covered with ſand, are forced upwards, and become viſible, and the wet marle in many places is ſqueezed through the ſand with them.

This appears to me to be the true reaſon of the ſinking of the ground at one place, and the riſing of the rocks at another.

That the reverend Mr. SACKETTE's account of the ſinking of the ground at Folkſtone, to the Royal Society, is founded in error, I have not the leaſt doubt, from the preſent appearance of ſome of the objects he deſcribes. I am rather at a loſs to follow him exactly, as the oldeſt man in the town of Folkſtone (I am told) never heard of the Mooring-rock he mentions.

I think by his deſcription the ſinking of the ground muſt have been in his time at the ſame place it is now, as Tarlingham-houſe is not to be ſeen on the other ſide of the town.

Admitting this to be the caſe, there will ſtill be a difficulty reſpecting the relative ſituation of each place in explaining what he calls a ſketch of the country. But, to explain my meaning more fully, let B (Tab. V.) repreſent the foot of the hill H in the view, which is upwards of 30 feet high.

CD, the valley between the hill B and the cliff.

DE, the cragged cliff, 60 yards high.

EF, a plain, above a mile long.

FG, a hill of ſteep aſcent, Mr. SACKETTE ſays near half a mile; but this is much higher than it really is.

GH, the land from the top of the hill to the houſe near a mile.

I, Tarlingham-houſe, lying two miles and a half N.N.W. from the rock.

EGH, a line of ſight (ſee Mr. SACKETTE's deſcription of the country).

If Mr. SACKETTE, in the above deſcription of his ſketch of the country, had placed each object according to its real ſituation; and if the effects he has mentioned had been real ones, they would have been truly wonderful, and worthy the attention of the curious inveſtigator of the hidden operations of nature; but I am apprehenſive he had but very little better foundation for what he has ſaid than the vague and inconſiſtent reports of a few ancient fiſhermen. Tarlingham-houſe is by Mr. SACKETTE's account ſituated full as far beyond the hill FG as the width of the plain EF; but how deep the hill has ſunk to render the houſe viſible over the top muſt depend upon the ſituation of it, *viz.* how much higher it was than the top of the hill.

If the hill has ſunk only ten feet, there muſt have been ſome external evidence of it, ſuch as fiſſures round the baſe, and a very ſteep aſcent from the top of it, where the ſeparation happened between it and Tarlingham-houſe; but there are no traces of any ſuch ſinking of the hills.

There is farther proof that Mr. SACKETTE did not examine into the matter himſelf, but reſted what he ſaid upon the report of others; and this is, that Tarlingham-houſe is not ſeen over the top of the hill in the line of ſight EG, but conſiderably to the left of it, in the line EI, and clear even of the baſe of the hill. Beſides, a moment's reflection would have told him, that the ſinking of the hills could not produce the effects he mentions; for if the ground in the plain was puſhed forward by it, it could not be a partial ſlipping; not only the church, and the whole town, muſt have been removed, but every object between the baſe of the hills and the cliff muſt have been removed out of their place; but I may venture to affirm, there is no proof of this having been done. I ſhould have been drawn into the ſame or ſimilar errors myſelf, if I had reſted ſatisfied with the firſt accounts I received from an ancient fiſherman. He told me the ſame ſtory of the hills ſinking in his time, and Tarlingham-houſe appearing higher than it did ſince he could remember. In one part of his relation he was right; for I found, upon inquiry, that Tarlingham-houſe has been taken down, and built upon a much larger ſcale than formerly, ſince it has been in the hands of the preſent proprietor.

If what I have ſaid ſhould not prove ſatisfactory, I ſhall be happy in giving you any farther information upon this ſubject in my power; and am, Sir, &c.

J. LYON.

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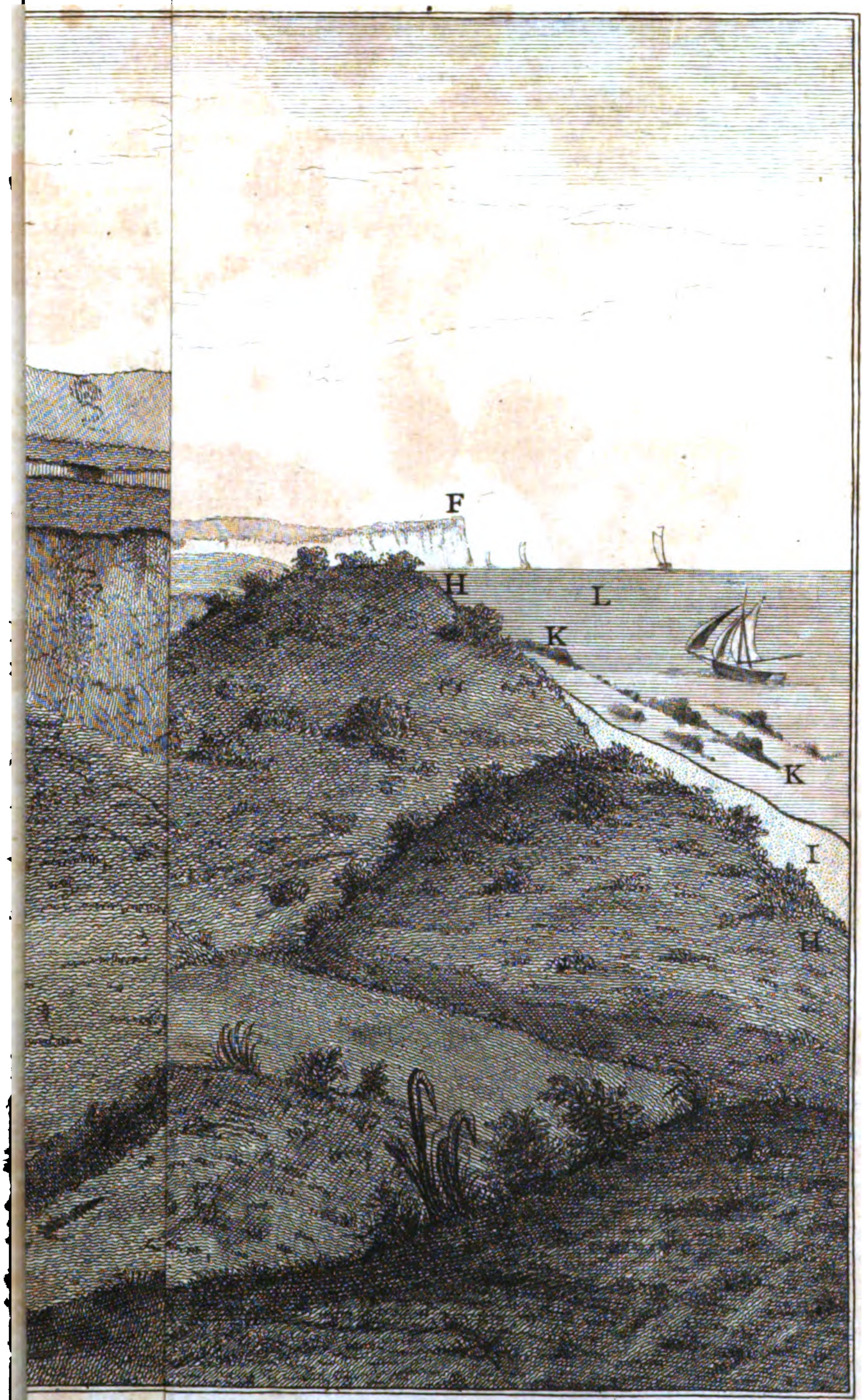

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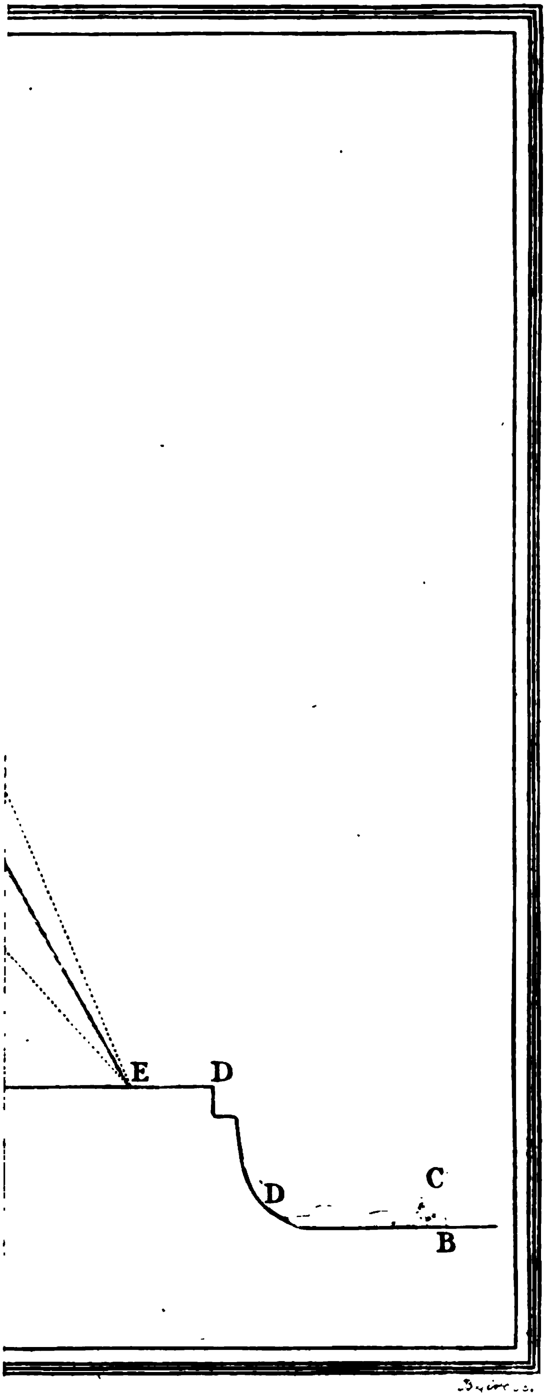

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XI. *Particulars relative to the Nature and Cuſtoms of the* Indians *of* North-America. *By Mr.* Richard M^c Cauſland, *Surgeon to the King's or Eighth Regiment of Foot. Communicated by* Joſeph Planta, *Eſq. Sec. R. S.*

Read February 16, 1786.

IT has been advanced by ſeveral travellers and hiſtorians that the Indians of America differed from other males of the human ſpecies in the want of one very characteriſtic mark of the ſex, to wit, that of a beard. From this general obſervation, the Eſquimaux have been excepted; and hence it has been ſuppoſed, that they had an origin different from that of the other natives of America. Inferences have alſo been drawn, not only with reſpect to the origin, but even relative to the conformation of Indians, as if this was in its nature more imperfect than that of the reſt of mankind.

It appears ſomewhat ſingular that authors, in deducing the origin both of the Eſquimaux and of the other Indians of America from the old world, ſhould never have explained to us how the former came to retain their beards, and the latter to lay them aſide. To aſcertain the authenticity of this point may perhaps prove of little real utility to mankind; but the ſingularity of the fact certainly claims the attention of the curious: and as it is impoſſible to fix any limits to the infe-

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rences which may at one time or another be drawn from alledged facts, it muſt always be of conſequence to inquire into the authenticity of thoſe facts, how little intereſting ſoever they may at preſent appear.

I will not by any means take upon me to ſay that there are not nations of America deſtitute of beards; but ten years reſidence at Niagara, in the midſt of the Six-Nations (with frequent opportunities of ſeeing other nations of Indians) has convinced me, that *they* do not differ from the reſt of men, in this particular, more than one European differs from another: and as this imperfection has been attributed to the Indians of North-America, equally with thoſe of the reſt of the Continent, I am much inclined to think, that this aſſertion is as void of foundation in one region as it is in the other.

All the Indians of North-America (except a very ſmall number, who, from living among white people, have adopted their cuſtoms) pluck out the hairs of the beard; and as they begin this from its firſt appearance, it muſt naturally be ſuppoſed, that to a ſuperficial obſerver their faces will ſeem ſmooth and beardleſs. As further proof that they have beards, we may obſerve, firſt, that they all have an inſtrument for the purpoſe of plucking them out. Secondly, that when they neglect this for any time, ſeveral hairs ſprout up, and are ſeen upon the chin and face. Thirdly, that many Indians allow tufts of hair to grow upon their chins or upper lips, reſembling thoſe we ſee in different nations of the old world. Fourthly, that ſeveral of the Mohocks, Delawares, and others, who live amongſt white people, ſometimes ſhave with razors, and ſometimes pluck their beards out. Theſe are facts which are notorious amongſt the Army, Indian-Traders, &c.; and which are never doubted in that part of the world by any perſon in the leaſt converſant with Indians: but as it is difficult to tranſport a matter of belief from one country to another diſtant one, and as the authors who have maintained the contrary opinion are too reſpectable to be doubted upon light grounds, I by no means intend to reſt the proofs upon what has been ſaid, or upon my ſingle aſſertion.

I have provided myſelf with two authorities, which I apprehend may in this caſe be deciſive. One is Colonel BUTLER, Deputy Superintendant of Indian Affairs, well known in the late American war, whoſe great and extenſive influence amongſt the Six-Nations could not have been acquired by any thing leſs than his long and intimate knowledge of them and their language. The other authority is that of THAYENDANEGA, commonly known by the name of Captain JOSEPH BRANT, a Mohock Indian of great influence, and much ſpoken of in the late war. He was in England in 1775, and writes and ſpeaks the Engliſh language with tolerable accuracy. I ſhall therefore only ſubjoin their opinions upon this matter, the originals of which I have under their own ſignatures.

Colonel BUTLER's.

THE men of the Six-Nation Indians have all beards naturally, as have all the other nations of North-America which I have had an opportunity of ſeeing. Several of the Mohocks ſhave with razors, as do likewiſe many of the Panees who are kept as ſlaves by the Europeans. But in general the Indians pluck out the beard by the roots from its earlieſt appearance; and as their faces are therefore ſmooth, it has been ſuppoſed that they were deſtitute of beards. I am even of opinion, that if the Indians were to practiſe ſhaving from their youth, many of them would have as ſtrong beards as Europeans.

(Signed)

Niagara, April 12, 1784. JOHN BUTLER.
Agent of Indian Affairs.

Captain BRANT's.

THE men of the Six-Nations have all beards by nature; as have likewiſe all other Indian nations of North America which I have ſeen. Some Indians allow a part of the beard upon the chin and upper lip to grow, and a few of the Mohocks ſhave with razors in the ſame manner as Europeans; but the generality pluck out the hairs of the beard by the roots as ſoon as they begin to appear; and as they continue this practice all their lives, they appear to have no beard, or at moſt only a few ſtraggling hairs which they have neglected to pluck out. I am however of opinion, that if the Indians were to ſhave they would never have beards altogether ſo thick as the Europeans; and there are ſome to be met with who have actually very little beard.

(Signed)
JOS. BRANT THAYENDANEGA.

Niagara, April 19, 1783.

Upon this ſubject I ſhall only further obſerve, that it has been ſuppoſed by ſome, that this appearance of beard on Indians ariſes only from a mixture of European blood; and that an Indian of pure race is intirely deſtitute of it. But the nations, amongſt whom this circumſtance can have any influence, bear ſo ſmall a proportion to the multitude who are unaffected by

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by it, that it cannot by any means be conſidered as the cauſe; nor is it looked upon as ſuch either by Captain BRANT or Colonel BUTLER.

I ſhall here ſubjoin a few particulars relative to the Indians of the Six-Nations, which, as they ſeem not to be well underſtood even in America, are probably ſtill leſs known in Europe. My authorities upon this ſubject, as well as upon the former, are the Indian Captain BRANT and Colonel BUTLER.

Each nation is divided into three or more tribes; the principal of which are called the Turtle-tribe, the Wolf-tribe, and the Bear-tribe.

Each tribe has two, three, or more chiefs, called Sachems; and this diſtinction is always hereditary in the family, but deſcends along the female line: for inſtance, if a chief dies, one of his ſiſter's ſons, or one of his own brothers, will be appointed to ſucceed him. Among theſe no preference is given to proximity or primogeniture; but the Sachem, during his life-time, pitches upon one whom he ſuppoſes to have more abilities than the reſt; and in this choice he frequently, though not always, conſults the principal men of the tribe. If the ſucceſſor happens to be a child, the offices of the poſt are performed by ſome of his friends until he is of ſufficient age to act himſelf.

Each of theſe poſts of Sachem has a name which is peculiar to it, and which never changes, as it is always adopted by the ſucceſſor; nor does the order of precedency of each of theſe names or titles ever vary. Nevertheleſs, any Sachem, by abilities and activity, may acquire greater power and influence in the nation

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than thoſe who rank before him in point of precedency; but this is merely temporary, and dies with him.

Each tribe has one or two chief warriors; which dignity is alſo hereditary, and has a peculiar name attached to it.

Theſe are the only titles of diſtinction which are fixed and permanent in the nation; for although any Indian may by ſuperior talents, either as a counſellor or as a warrior, acquire influence in the nation, yet it is not in his power to tranſmit this to his family.

The Indians have alſo their *Great Women* as well as their *Great Men*, to whoſe opinions they pay great deference; and this diſtinction is alſo hereditary in families. They do not ſit in council with the Sachems, but have ſeparate ones of their own.

When war is declared, the Sachems and great Women generally give up the management of public affairs into the hands of the warriors. It may however ſo happen, that a Sachem may at the ſame time be alſo a chief warrior.

Friendſhips ſeem to have been inſtituted with a view towards ſtrengthening the union between the ſeveral nations of the confederacy; and hence *Friends* are called the ſinews of the Six-Nations. An Indian has therefore generally one or more *friends* in each nation. Beſides the attachment which ſubſiſts during the life-time of the two friends, whenever one of them happens to be killed, it is incumbent on the ſurvivor to *replace* him, by preſenting to his family either a ſcalp, a priſoner, or a belt conſiſting of ſome thouſands of wampum; and this ceremony is performed by every *friend* of the deceaſed.

The purpoſe and foundation of war parties therefore, is in general, to procure a priſoner or ſcalp to replace the friend or relation of the Indian who is the head of the party. An Indian who wiſhes to replace a friend or relation preſents a belt to his acquaintance, and as many as chuſe to follow him accept this belt, and become his party. After this, it is of no conſequence whether he goes on the expedition or remains at home (as it often happens that he is a child), he is ſtill conſidered as the head of the party. The belt he preſented to his party is returned fixed to the ſcalp or priſoner, and paſſes along with them to the friends of the perſon he replaces. Hence it happens, that a war party, returning with more ſcalps or priſoners than the original intention of the party required, will often give one of theſe ſupernumerary ſcalps or priſoners to another war party whom they meet going out; upon which this party, having fulfilled the purpoſe of their expedition, will ſometimes return without going to war.

XII. *Abſtract of a Regiſter of the Barometer, Thermometer, and Rain at* Lyndon *in* Rutland, *in* 1785. *By* Thomas Barker, *Eſq. Alſo of the Rain at* South Lambeth, *in* Surrey; *and at* Selbourn *and* Fyfield, Hampſhire. *Communicated by* Thomas White, *Eſq. F.R.S.*

Read February 23, 1786.

		Barometer.			Thermometer.						Rain.			
					In the Houſe.			Abroad.			Lyndon	S. Lambeth.	Selbourn.	Fy
		Higheſt	Loweſt	Mean.	High.	Low.	Mean	High.	Low.	Mean				
		Inches.	Inches.	Inches.	°	°	°	°	°	°	Inch.	Inch.	Inch.	In
Jan.	Morn.	29,83	28,59	29,31	45½	34½	39	45	25½	35	1,494	1,78	2,84	2,
	Aftern.				46	34½	40	47½	27	38½				
Feb.	Morn.	30,16	28,45	29,34	39	29	34½	38	10	28	0,365	1,20	1,80	1,
	Aftern				40	30½	36	42½	23	34				
Mar.	Morn.	29,84	29,16	29,61	44	29½	36	43½	17	30½	0,212	0,35	0,30	0,
	Aftern.				45	31½	37½	51	27½	38				
Apr.	Morn.	30,05	28,88	29,71	56	36	48	52½	25	41	0,175	0,34	0,17	0,
	Aftern.				58	37	50	67½	37½	54				
May	Morn.	30,09	28,95	29,54	61	50½	55	58½	42	48	0,666	0,81	0,60	0,
	Aftern.				64	51½	56½	75½	50½	60½				
June	Morn.	29,99	29,32	29,71	66½	53	61	60	48	55	1,567	2,04	1,39	1,
	Aftern.				69½	55½	63	80½	54½	69				
July	Morn.	29,82	28,97	29,42	68½	60	64	64	53½	58½	3,283	1,73	3,80	1,
	Aftern.				73	61	65	83	60½	70				
Aug.	Morn.	29,72	28,99	29,36	64½	55½	59½	59½	43½	53½	4,315	3,05	3,21	4,
	Aftern.				65	56½	61	71	55	64				
Sept.	Morn.	29,89	28,51	29,29	63	50	59	59	36	52	3,314	2,75	5,94	5,
	Aftern.				64½	51½	60	72	47	63				
Oct.	Morn.	29,99	28,95	29,45	58	41½	50½	55½	28	42½	1,653	4,04	5,21	2,
	Aftern.				59½	43	51½	62½	37½	52				
Nov.	Morn.	30,02	28,33	29,33	51½	39	44½	49½	28	37	1,125		2,27	1,
	Aftern.				52½	39½	45	56½	34½	44				
Dec.	Morn.	29,79	28,76	29,32	43½	32	39	43	20½	33	2,037	1,53	4,02	3,
	Aftern.				44½	32½	40	45½	24	37				
										Inches	20.206	19,62	31,55	2

The ſevere froſt of December, 1784, broke early in January, and was all gone before the middle, and the moſt open part of this ſharp winter followed it, being miſty or thick and warm very wet air; but the laſt day of January another froſt ſet in, which, though not ſo ſteady as the former, was ſometimes very ſevere, and did not go away till near the middle of March: and this winter, particularly the former froſt about December 10, was much ſeverer in the ſouth of England than here, and greater ſigns of deſtruction by it were ſeen among the trees and plants there. From the breaking of the froſt till April 4, was chiefly froſty mornings, and ſometimes in the ſhade all day; ſo that, if you count the number of froſty days, I do not know that any winter had more, though I have known ſeveral longer froſts, and more ſteady, and ſome few more ſevere.

From April 5, the weather began to mend, was tolerably pleaſant, and things came on gradually; yet not without ſome froſty mornings, even in May. The ſeed time began late, but was without hindrance; and there having been very little rain ſince the froſt, it harrowed remarkably fine, and the lands and roads were uncommonly duſty. The corn came up very well, except the late ſown, ſome of which, eſpecially in the ſouth of England, lay dry till June; for it continued a remarkably dry time all ſpring, ſo that the graſs was very ſhort, and hay very ſcarce; yet the grain continued particularly fine-coloured, and eared very well, though ſome of the winter corn was rather thin; yet that was much mended by ſome refreſhing ſhowers in May and June, which were enough to freſhen things, though not to make much graſs: and during this drought there were great numbers of little whirlwinds, ſometimes ſeveral in a day.

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The weather began to be ſhowery the middle of July, and ſeveral great rains; and after Auguſt 3d it was more frequent, but leſs at a time. This made plenty of good graſs, but was very troubleſome for the harveſt, which was got in ſlowly, and with loſs, but came out again full as well as could be expected. The wheat was remarkably full-eared. The barley good, except the late ſown, which never ripened; and ſome too haſtily carried in harveſt. The birds of paſſage went away rather early this year: almoſt all the Swifts were gone in July, and moſt of the Swallows and Martins in September; the laſt were Auguſt 7, and October 12. It continued very ſhowery till near the middle of October; after which the autumn was pretty fine, and leſs wet than before, yet enough to make it very dirty when the ſun loſt its power in December; and the winter began for the moſt part open and pleaſant, till a froſt and large ſnow at Chriſtmas, which grew ſeverer to the end of the year.

On the Variations of Seaſons.

Meaſuring the rain for a few years will not ſhew completely the general quantity of rain which falls in any place; for there is a very great difference at different periods of time. If I had meaſured the rain at Lyndon only in the four years 1740, 1741, 1742, 1743, the mean would have been found to be only 16½ inches in a year; yet they were not all complained of as dry ſummers. 1740 was cold and dry till July 30. The ſpring 1741 was cold and dry, the ſummer hot, dry, and burning till the beginning of September; then ten days wet and very warm again, being the fineſt autumn for graſs ever known. 1742 was a ſhowery ſummer, and 1743 wet in the middle; but then the winters were dry, ſo that the quantity of rain upon the whole was ſmall. 1741 to 1750 the mean was 18½ inches. 1741 and 1750 were hot, dry, and burning, 1750 being the hotteſt year I have known. The intermediate years were neither very wet nor very dry; and this was the moſt plentiful and cheapeſt time for corn of any ten years I remember; for grain oftener fails in England from too much wet than too little. 1751 to 1760 the mean year was 22¼. 1760 was hot, dry, and burning; but ſeveral of the ſummers were wet, and the crops not ſo plentiful. Three wet ſummers together, 1754, 1755, and 1756, were a time of ſcarcity, and we have had more failing crops ſince that time than before it. From 1761 to 1770 there was 23¼ in a year. 1762 was hot, dry, and burning; and 1765 cold and dry; but ſeveral years were wet, 1763 and 1768 remarkably ſo; and of thoſe ten years ſeveral had failing crops, and ſome had great ſnows. There was a great change of the ſeaſons at 1763; for I have had more rain ſince that time than I had before it in the proportion of 5 to 4. From 1770 to 1780 there was at a mean 26 inches. 1771 was dry, and 1778 and 1779 were hot, yet not without fits of rain; and moſt of the other years were wet, and ſome great ſnows. 1773, 1774, and 1775, were ſo wet that there came 32 inches in a year, which is nearly double what there was from 1740 to 1743. In twelve months, from October 1773, to September 1774, there came 39,390 inches of rain, which is nearly a Lancaſhire year. And in one month, September 1774, there was 8 inches: this was in barley and peaſe harveſt, and for three weeks together not a load could be carried in. By the above ſtate of the caſe it appears, that, for four ſucceſſive periods of ten years, the quantity of rain has been increaſing each time.

XIII. *An Account of Experiments made by Mr.* John M^c Nab, *at* Henley Houſe, Hudſon's Bay, *relating to freezing Mixtures.* *By* Henry Cavendiſh, *Eſq. F. R. S. and A. S.*

Read February 23, 1786.

IN my obſervations on Mr. HUTCHINS's Experiments, printed in the LXXIIId volume of the Philoſophical Tranſactions, I gave my opinion concerning the cauſe of the cold produced by mixing ſnow with different liquors. As there were ſome circumſtances, however, which ſeemed to form a difficulty in the way of this opinion, I was deſirous of having further experiments made on the ſubject; and at the ſame time I thought that, by proper management, a greater degree of cold might be produced than had hitherto been done. On mentioning the experiments I wiſhed to have made to Mr. HUTCHINS, he very obligingly deſired Mr. M^c NAB, Maſter at Henley-Houſe, to try them; who was ſo good as to undertake the buſineſs, and has executed it in the moſt ſatisfactory manner; as he has not only taken great pains, but has ſhewn the utmoſt attention and accuracy, in obſerving and relating all the phænomena which occurred, and has manifeſted great judgement in frequently adapting the manner of trying the experiments to appearances which occurred in former ones, to which we are indebted for great part of the moſt curious facts in this paper. His endeavours have alſo been attended with much ſucceſs, as he has not only ſhewn many remarkable circumſtances relating to the freezing of the nitrous and vitriolic acids, and the phænomena of freezing mixtures; but has alſo produced degrees of cold greatly ſuperior to any before known.

1. In the above-mentioned Paper I ſaid, that the cold produced by mixing ſpirit of nitre with ſnow, is owing to the melting of the ſnow; and that in all probability there is a certain degree of cold, in which ſpirit of nitre is ſo far from diſſolving ſnow, that it will yield out part of its own water, and ſuffer that to freeze, as is the caſe with ſolutions of common ſalt; ſo that if the cold of the materials, before mixing, is equal to this, no additional cold can be produced. A circumſtance, however, which at firſt ſight ſeems repugnant to this opinion, occurred in an experiment of FAHRENHEIT's for producing cold by a mixture of ſpirit of nitre and ice; namely, that the acid, which had been repeatedly cooled by different frigorific mixtures, was found frozen before it was mixed with the ice; notwithſtanding which, cold was produced by the mixture. Profeſſor BRAUN alſo found, that cold was produced by mixing frozen ſpirit of nitre with ſnow. On conſideration, however, this appeared by no means inconſiſtent with the opinion there laid down, as there was great reaſon to think, that the freezing of the acid was of a different kind from that conſidered in the above-mentioned Paper, and that it did not proceed from the watery part ſeparating from the reſt and freezing; but that the whole acid, or perhaps the more concentrated part, froze; in which caſe it would not be extraordinary that the acid ſhould diſſolve more ſnow, and produce cold.

2. To clear up this point, I ſent to Hudſon's Bay a bottle of ſpirit of nitre, of nearly the ſame ſtrength as FAHRENHEIT's; and deſired Mr. M^c^ NAB to expoſe it to the cold, and, if it froze, to aſcertain the temperature, and decant the fluid part into

another bottle, and ſend both home to be examined, as it would thereby be known, whether it was the whole acid, or only the watery part, which froze. For the ſame purpoſe alſo I ſent ſome dephlogiſticated ſpirit of nitre of the ſame ſtrength, and alſo ſome ſtrong oil of vitriol. I alſo ſent ſome ſpirit of nitre and ſpirit of wine, both diluted with ſo much water, that it was expected, that with the cold of Hudſon's Bay they would ſuffer the firſt kind of congelation; that is, their watery part would freeze, and thereby make the difference between the two kinds of freezing more apparent.

3. In the ſame Paper I ſay, "That on adding ſnow gra- "dually to ſome of the ſpirit of nitre uſed by Mr. HUTCHINS, "I found, that the addition of a ſmall quantity produced heat "inſtead of cold; and it was not until ſo much was added as to "increaſe the heat from 28° to 51°, that the addition of more "ſnow began to produce cold; the quantity of ſnow required "for this purpoſe being pretty exactly one quarter of the "weight of the ſpirit of nitre, and the heat of the ſnow and "air of the room, as well as the acid, being 28°. The reaſon "of this is, that a great deal of heat is produced by mixing "water with ſpirit of nitre, and the ſtronger the ſpirit is, the "greater is the heat produced. Now it appears from this "experiment, that before the acid was diluted, the heat "produced by its union with the water formed from the melted "ſnow was greater than the cold produced by the melting of "the ſnow; and it was not till it was diluted by the addition "of one quarter of its weight of that ſubſtance, that the cold "generated by the latter cauſe began to exceed the heat gene- "rated by the former. From what has been ſaid, it is evi- "dent, that the cold of a freezing mixture, made with the "undiluted acid, cannot be quite ſo great as that made with

"the ſame acid, diluted with a quarter of its weight of water, "ſuppoſing the acid and ſnow to be both at 28° of heat; and "there is no reaſon to think, that the event will be different if "they are colder; for the undiluted acid will not begin to "generate cold, until ſo much ſnow is diſſolved as to increaſe "its heat from 28° to 51°, ſo that no greater cold will be "produced, than would be obtained by mixing the diluted acid "heated to 51° with ſnow of the heat of 28°. This method "of adding ſnow gradually to an acid, is much the beſt way "I know of finding what ſtrength it ought to be of, in order "to produce the greateſt effect poſſible."

As it ſeemed likely that, by following this method, a greater degree of cold might be produced than had been done hitherto, I ſent three other bottles of ſpirit of nitre and oil of vitriol, all three diluted, but not ſo much ſo, but that I thought they would require a little further dilution, in order to reduce them to their propereſt degree of ſtrength. I alſo ſent a bottle of highly rectified ſpirit of wine, and a mixture of equal quantities of the above-mentioned common ſpirit of nitre and oil of vitriol; and deſired Mr. M^c^ NAB to find what degree of cold could be produced by mixing them with ſnow, after having firſt reduced them, in the above-mentioned manner, to their beſt degree of ſtrength *.

He was alſo deſired to aſcertain how much ſnow he added; for as their ſtrength was determined before they were ſent out, it would thereby be known what was the beſt ſtrength of theſe liquors for frigorific mixtures.

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* This might have been done at home; but I thought it not unlikely that the ſtrength found this way might differ, in ſome meaſure, according to the heat in which the experiment was tried.

All theſe bottles were numbered with a diamond; and as I ſhall ſometimes diſtinguiſh them by theſe numbers, and as it may be of uſe to thoſe who may conſult the original, I have added the following liſt of theſe bottles, with their contents.

N°	Liquors mentioned in Art. 3.	Weight of marble which they diſſolve.	Specific gravity at 60° of heat.
168	Spirit of nitre, ...	,582	1,4371
27	Dephlogiſticated ſpirit of nitre, ...	,53	1,4040
103	Diluted oil of vitriol, ...	,654	1,5596
28	Equal weights of N° 168. and N° 103. . .	– – –	– –
8	Very highly rectified ſpirit of wine, . . .	– – –	,8195
	Liquors mentioned in Art. 2.		
151	Strong oil of vitriol, ...	,98	1,8437
142	Spirit of nitre, ...	,525	1,4043
139	Some of the ſame diluted with twice its weight of water, ...	– – –	– –
141	Dephlogiſticated ſpirit of nitre, ...	,53	1,4033
143	Some of the ſame ſpirit of wine as in N° 8. diluted with 1½ its weight of water, .	– – –	– –
72	Diluted oil of vitriol for comparing the thermometers,	,629	– –
171	Oil of vitriol of about the uſual ſtrength, but the exact ſtrength not known, intended to refreſh the former when too weak.		

4. Profeſſor Braun ſays, that by mixtures of ſnow and ſpirit of nitre he ſunk thermometers filled with oil of ſaſſafras, and ſome other eſſential oils, to – 100° or – 124°; and that, by the ſame means, he ſunk thermometers filled with the higheſt rectified ſpirit of wine to – 148°. Though there ſeemed great reaſon to think, from Mr. Hutchins's experiments, that there muſt be ſome miſtake in this; yet, as it was poſſible that the eſſential oils, and even ſpirit of wine of a ſtrength much different from that with which Mr. Hutchins's thermometers were filled, might follow a conſiderably different progreſſion in their

contraction by great degrees of cold, I ſent a thermometer filled with oil of ſaſſafras, and two others with ſpirits of wine. One of theſe laſt was filled with the higheſt rectified ſpirits I could procure, its ſpecific gravity at 60° of heat being ,8185; the other was intended to be filled with common ſpirits, though from circumſtances I am inclined to ſuſpect *that* alſo to have been filled with the beſt ſpirits. Beſides theſe, there was ſent a mercurial thermometer, accurately adjuſted, according to the directions of the Committee of the Royal Society, printed in the LXVIIth volume of the Tranſactions; and alſo the two ſpirit thermometers uſed by Mr. HUTCHINS, which were filled with ſpirits whoſe ſpecific gravity was ,8247.

5. Theſe thermometers were compared together by expoſing them to the cold, with their balls immerſed in a glaſs veſſel filled with diluted oil of vitriol. They were at times alſo compared in cold more violent than the natural cold of the climate, by adding ſnow to the acid in which they were tried, in which caſe care was taken to keep the mixture frequently ſtirred. Oil of vitriol was recommended for this purpoſe, as a fluid which would moſt likely bear any degree of cold without freezing, and whoſe natural cold might be much increaſed by the addition of ſnow. It ſeems to have anſwered the purpoſe very well, and not to have been attended with any inconvenience.

During the firſt compariſon of theſe thermometers, a whitiſh globule, ſuch as thoſe which appear in frozen oil, was obſerved in the tube of the thermometer filled with oil of ſaſſafras. This appearance of congelation did not much increaſe; but two days after a large air bubble was found in its ball, which prevented Mr. M^c NAB from making further obſervations with it.

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It is well known, that ſpirit of wine expands more by a given number of degrees of a mercurial thermometer in warm temperatures than in cold ones; and this inequality, as might be expected, was leſs in the ſtronger ſpirit than in the weaker, but the difference was inconſiderable. The oil of ſaſſafras alſo had ſome of this inequality, but much leſs. It however appears to be by no means a proper fluid for filling thermometers with. No appearance was obſerved which indicates any conſiderable irregularity in the contraction of ſpirits of wine in intenſe cold, or which renders it probable, that thermometers filled therewith could be ſunk by a mixture of ſnow and ſpirit of nitre to a degree near approaching to that mentioned by Profeſſor BRAUN.

6. Mr. M^c^ NAB in his experiments ſometimes uſed one thermometer and ſometimes another; but in the following pages I have reduced all the obſervations to the ſame ſtandard; namely, in degrees of cold leſs than that of freezing mercury I have ſet down that degree which would have been ſhewn by the mercurial thermometer in the ſame circumſtances; but as that could not have been done in greater degrees of cold, as the mercurial thermometer then becomes of no uſe, I found how much lower the mercurial thermometer ſtood at its freezing point, than each of the ſpirit thermometers, and increaſed the cold ſhewn by the latter by that difference.

On the common and dephlogiſticated Acids of Nitre.

The following experiments ſhew, that both theſe acids are capable of a kind of congelation, in which the whole, and not merely the watery part, freezes. Their freezing point alſo differs greatly according to the ſtrength, and varies according to a very unexpected law. Like water too they bear being cooled very much below their freezing point before the congelation begins, and as ſoon as that takes place, immediately riſe up to the freezing point.

7. On the morning of Feb. 1. the common and dephlogiſticated ſpirits of nitre, N° 142 and 141, whoſe ſpecific gravities were 1,4043 and 1,4033, were found clear and fluid, the cold of the air at that time being − 47°. They alſo bore being ſhook without any alteration; but on taking out their ſtoppers, both of them in a few minutes began to freeze, the congelation beginning by a white appearance at top, which gradually ſpread to the bottom; and they became ſo thick as not to move on inclining the phial. For want of a thermometer whoſe ball reached far enough below its ſcale, Mr. M^c^ NAB was not able to determine their cold while in the bottle; but in ſomewhat more than an hour's time, the frozen acid had ſo much ſubſided as to admit of his pouring a little fluid matter out of each into a glaſs with a thermometer in it*; whereby the cold of the common ſpirit of nitre was found to be − 31°½, and that of the dephlogiſticated acid − 30°, the temperature of the air being − 41°. Each of theſe decanted liquors, at the time their temperature was tried, was full of ſmall *ſpicula* of ice: they were then put into phials well ſtopped, and they, as well as the undecanted liquors, ſent home to be examined. The decanted part of the common

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* It may be aſked, why it was more poſſible to decant any liquor at this time than at firſt, as the acid was all the while expoſed to a cold much below the freezing point? The reaſon in all probability is, not that any part of the ice firſt formed diſſolved, but that the ſmall filaments into which it ſhot collected together, and in ſome meaſure ſubſided to the bottom.

ſpirit of nitre diſſolved ,535 of its weight of marble, and the undecanted part ,523; for which reaſon I ſhall call the ſtrength of the former ,535, and that of the latter ,523; which mode of reckoning is obſerved in the remainder of this Paper. The ſtrength of the decanted part of the dephlogiſticated acid was ,56, and that of the undecanted part ,528; ſo that it appears that in each of theſe acids the unfrozen part was a little ſtronger than the frozen part. It is remarkable, that in the common ſpirit of nitre, the decanted part, though ſtronger than the other, was paler coloured and leſs fuming.

8. On Dec. 21, the temperature of the air being −28°, ſome dephlogiſticated ſpirit of nitre (N° 27.) of nearly the ſame ſtrength as the former acid, was poured into a jar, in order to be diluted with ſnow, as recommended in Art. 2. Immediately after it was decanted, it began to freeze, in the ſame manner as before deſcribed, except that a leſs portion of it ſeems to have congealed: its temperature, tried by dipping a thermometer into it, was −19°, where it remained ſtationary for many minutes; it was then diluted with ſnow, as will be mentioned in Art. 14. whereby its ſtrength was reduced to ,434.

9. On Dec. 29th, this diluted acid was completely melted, and half of it poured into a jar with a ground ſtopper, and both portions expoſed to the air. In the morning they were perfectly fluid; but on taking the ſtopper out of the jar, and dipping in it a thermometer, the acid immediately froze, beginning by forming a white coat round the ball of the thermometer, which gradually ſpread through the whole fluid; and at the ſame time the thermometer roſe till it ſtood ſtationary at −5°. The cold of the acid before it began to freeze muſt have been about −30°½, that being the temperature of a

glaſs of vitriolic acid ſtanding near it; but the thermometer which was dipped into it was five or ſix degrees colder, which ſeems to be the cauſe of the congelation beginning round the ball.

In the afternoon a thermometer was dipped into the other half of the acid, where, as the weather had grown leſs cold, it ſtood above a minute at – 25°, without freezing; then, however, the acid froze, with the ſame appearance as in the morning, and at the ſame time the thermometer roſe to – 4°, and became ſtationary.

This acid, being left in the air with the thermometer in it, was found in the evening at – 45°; it however was not intirely frozen, being only thick as an unguent, which ſhews that the unfrozen part muſt have been of a different ſtrength from the frozen part; but it does not appear whether ſtronger or weaker. The next morning it was frozen ſolid, though the cold was only half a degree greater.

On Jan. 16th, this acid was again tried in the ſame manner; it then ſuffered a thermometer, whoſe ball had been previouſly warmed in the hand, to be dipped into it, and remain there ſeveral minutes without freezing, though its temperature was – 35°. But on lifting up the thermometer, a drop fell from its ball into the acid, which immediately ſet it a freezing, and it roſe up to – 4°½.

10. On Dec. 22d, the ſpirit of nitre (N° 168.) which a few days before had been diluted with ſnow, ſo as to be reduced to the ſtrength of ,411, was divided into two equal parts, and expoſed to the cold. On Dec. 29th, when the temperature of the air was – 17°½, one of theſe parts was found beginning to freeze; the other was fluid, but began to freeze on dipping in a thermometer; the thermometer in both kept ſtationary at – 1°

− 1°½. The latter was twice re melted and expoſed to the cold, and both times the temperature of the frozen acid came out the ſame as before.

11. The white colour of the ice in theſe experiments ſeems owing only to its conſiſting of very ſlender filaments; for in ſome caſes, where it froze ſlower, and where, in conſequence, it ſhot into larger ſolid maſſes, they were tranſparent, and of the ſame colour as the acid itſelf. By the continuance of a ſufficient cold, the acid, which by haſty freezing put on the white appearance, would become hard ſolid ice, but yet ſtill retained its white appearance, owing perhaps to the filaments firſt ſhot conſiſting of an acid differing in ſtrength from that which froze afterwards, and filled up the interſtices.

In all theſe experiments, whether the ice was formed into minute filaments or ſolid maſſes, ſtill, whenever there was a ſufficient quantity of fluid matter to admit of it, they conſtantly ſubſided to the bottom; a proof that the frozen part was heavier than the unfrozen. The difference indeed is ſo great, that in one caſe where it froze into ſolid cryſtals on the ſurface, theſe cryſtals, when detached by agitation, fell with force enough to make a tinkling noiſe againſt the bottom of the glaſs.

Theſe acids contract very much on freezing. Whenever the acid is frozen ſolid, the ſurface, inſtead of being elevated in ridges, like frozen water, is depreſſed and full of cracks. In one experiment Mr. M^c^ Nab, after a glaſs almoſt full of acid was nearly frozen, filled it to the brim with freſh acid; and then, after it was completely frozen, the ſurface was viſibly depreſſed, with fiſſures one-eighth of an inch broad, extending from top to bottom. It is this contraction of the acid in freezing which makes the frozen part ſubſide in the fluid part; as it was found, in the undiluted acid, that the latter conſiſted of a ſtronger, and conſequently heavier, acid than the former. But ſtill the ſubſidence of the frozen part ſhews, that the ice is not mere water, or even a very dilute acid; which indeed was proved by the examination of the liquors ſent home.

The ninth and tenth articles ſhew, that though the acids bear being cooled greatly below the freezing point, without any congelation taking place, yet as ſoon as they begin to freeze they immediately riſe up to their freezing point; and this point is always very nearly, if not exactly, the ſame in the ſame acid; for thoſe acids were frozen and melted again three or four times, and were cooled conſiderably more below the freezing point in one trial than another, and yet as ſoon as they began to freeze the thermometer immerſed in them conſtantly roſe nearly to the ſame point.

The quantity which theſe acids will bear being cooled below the freezing point, without freezing, is remarkable. The diluted ſpirit of nitre, whoſe freezing point is – 1°½, once bore being cooled to near – 39°, without freezing, that is, near 37 degrees below its freezing point. The diluted dephlogiſticated ſpirit of nitre, whoſe freezing point is – 5°, bore cooling to – 35°; and the dephlogiſticated ſpirit of nitre (141) whoſe true freezing point is moſt likely – 19° *(ſee next article)* bore being cooled to – 49°: perhaps too they might have born to be cooled conſiderably lower without freezing, but how much does not appear. It muſt be obſerved, however, that the ſame diluted ſpirit which at one time bore being cooled to – 39°, at another froze, without any apparent cauſe, when its cold was certainly leſs than – 30°, and moſt likely not much below – 18°.

12. The freezing point differs remarkably, according to the ſtrength of the acid. In the diluted dephlogiſticated and common ſpirit of Art. 7. and 8. the freezing point was $-5°$ and $-1°\frac{1}{2}$. In the dephlogiſticated and common ſpirit of Art. 5. the decanted parts of which were ſtronger than the foregoing in ſcarcely ſo great a proportion as that of four to three, it ſeemed to be $-30°$ and $-31°\frac{1}{2}$. It may indeed be ſuſpected, that as this point was determined only by pouring a ſmall quantity of the acid into a glaſs, at a time when the air and glaſs were much colder than the acid, theſe decanted liquors might be cooled by the air and glaſs, and thereby make the freezing point appear lower than it really was: but I do not think this could be the caſe; for as the decanted liquors were full of ſmall filaments of ice, they could hardly be cooled ſenſibly below their freezing points without freezing; and any cold, communicated to them by the air or glaſs, would ſerve only to convert more of them into ice, without ſenſibly increaſing their cold: ſo that I think this experiment determines the true freezing point of their decanted part; but it muſt be obſerved, that as the decanted part was rather ſtronger than the reſt, it is very poſſible that the freezing point of the undecanted part might be conſiderably leſs cold.

A circumſtance which might incline one to think, that the way by which the freezing point was determined in this experiment is defective is, that the freezing point of the dephlogiſticated acid N° 27. though nearly of the ſame ſtrength as that laſt mentioned, but rather ſtronger, was much leſs low, being only $-19°$. But I have little doubt that the true reaſon of this is, that in the former acid the ſtrength of the decanted part, which is the part whoſe freezing point was tried, was found to be at leaſt $\frac{1}{20}$ greater than that of the whole maſs; whereas in N° 27. the fluid part was in all probability not ſenſibly ſtronger than the whole maſs; for as N° 27. was cooled only ſeven degrees below the freezing point, and its temperature was tried ſoon after its beginning to freeze, not much of the acid could have frozen; whereas the other was cooled 15 degrees below its freezing point, and was expoſed for an hour or two to an air not much leſs cold, in conſequence of which a conſiderable part of the acid muſt have frozen; ſo that in all probability the acid, whoſe freezing point was found to be − 30°, was in reality $\frac{1}{10}$ part ſtronger than that whoſe freezing point was − 19°.

If this reaſoning be juſt, the freezing point of theſe acids is as follows:

		Freezing point.
Dephlogiſticated ſpirit of nitre, whoſe ſtrength =	,56	− 30°
	,53	− 19
	,437	− 4½
Common ſpirit of nitre, whoſe ſtrength =	,54	− 31½
	,411	− 1¼

On the Phænomena obſerved on mixing Snow with theſe Acids.

13. On Dec. 13, ſnow was added to the ſpirit of nitre N° 168, as recommended in Art. 2. The ſnow was put in very gradually, and time was taken to find what effect each addition had on the thermometer and mixture, before more was added. The temperature of the acid before the mixture was − 27°, and each addition of ſnow raiſed the thermometer a little, till it roſe to − 1°¼; after which the next addition made it ſink to − 2°, which ſhewed that ſufficient ſnow had then been added. The quantity

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quantity of ſnow uſed was pretty exactly $\frac{4}{10}$ of the weight of the acid, the weight of the acid being 13 oz. ſo that the ſtrength of the diluted acid was reduced to ,411.

The acid before the addition of ſnow had no ſigns of freezing, its temperature being in all probability much above its freezing point; yet the ſnow did not appear to diſſolve, but formed thin white cakes, which however did not float on the ſurface, but fell to the bottom, and when broke by the ſpatula formed a gritty ſediment; ſo that it appears, that theſe cakes are not ſimply undiſſolved ſnow, but that the adjoining acid abſorbed ſo much of the ſnow in contact with it, as to become diluted ſufficiently to freeze with that degree of cold, and then congealed into theſe cakes. The quantity of congealed matter ſeems to have kept increaſing till the end of the experiment.

14. On Dec. 21, an experiment was made in the ſame manner with the dephlogiſticated ſpirit of nitre N° 27. The acid began to freeze in pouring it into the jar in which the mixture was to be made, and ſtood ſtationary there at – 19°, as related in Art. 6.; ſo that the liquor at the beginning of the experiment was white and thick, which made the effect of the addition of the ſnow leſs ſenſible. However, the congealed matter conſtantly ſubſided to the bottom, and the quantity ſeems to have continued increaſing to the end of the experiment. The heat of the mixture roſe to – 4° before cold began to be produced, and the quantity of ſnow added was $\frac{22}{100}$ of that of the acid, ſo that the ſtrength of the acid was reduced to ,437 by the dilution.

A very remarkable circumſtance in this experiment is, that the acid, while the ſnow was adding, firſt became of a yellowiſh,

lowiſh, and afterwards of a greeniſh or bluiſh hue. This colour did not go off by ſtanding, but continued at leaſt ten days, during which time the acid conſtantly kept that colour, except when by haſty freezing it ſhot into ſmall filaments, in which caſe it put on the white appearance which theſe acids always aſſumed under thoſe circumſtances; but once that by gradual freezing it ſhot into tranſparent ice, this ice was of a bluiſh colour.

It is difficult to conceive what this colour ſhould proceed from. Spirit of nitre is well known to aſſume this colour when much phlogiſticated and properly diluted; but one does not ſee why it ſhould become phlogiſticated by the addition of the ſnow, and ſtill leſs why the dephlogiſticated acid ſhould become more phlogiſticated thereby than the common acid did; for though it is not extraordinary, that a proceſs not capable of producing any increaſe of phlogiſtication in the common acid, ſhould make this as much phlogiſticated as that, yet it is very extraordinary that it ſhould make it more ſo. No notice is taken of any efferveſcence or diſcharge of air while it was aſſuming this colour, nor was it obſerved that it became more ſmoking thereby, or that the top of the phial in which it was kept became full of red fumes, as might naturally be expected if it was rendered much phlogiſticated. Theſe are circumſtances which, conſidering Mr. M^c^ NAB's great attention to ſet down all the phænomena that occurred, I ſhould think would hardly have been omitted if they had really happened.

15. It is remarkable, that in both theſe experiments the addition of ſnow produced heat, until it arrived pretty exactly at what was found to be the freezing point of the diluted acid; but that as ſoon as it arrived at that point, the addition of more ſnow began to produce cold. This can hardly be owing

merely to accident, and to both acids having happened to be of that preciſe degree of heat before the experiment began, that their heat after dilution ſhould coincide with the freezing point anſwering to their new ſtrength. The true cauſe ſeems to be as follows. It will be ſhewn in Art. 16. and 17. that the freezing point of theſe acids, when diluted as in the foregoing experiments, is much leſs cold than when they are conſiderably more diluted; and it was before ſhewn to be much leſs cold than when not diluted; ſo that there muſt be a certain degree of ſtrength, not very different from that to which theſe acids were reduced by dilution, at which they freeze with a leſs degree of cold than when they are either ſtronger or weaker. Now in theſe experiments, the temperature of the liquors before dilution was below this point of eaſieſt freezing, and a great deal of the acid was in a ſtate of congelation all the time of dilution; the conſequence of which is, that when they were diluted to the ſtrength of eaſieſt freezing, they would alſo be at the heat of eaſieſt freezing; for they could not be below that point, becauſe, if they were, ſo much of the acid would immediately freeze as would raiſe them up to it; and they could not be above it, for, if they were, ſo much of the congealed acid would diſſolve as would ſink them down to it. After they were arrived at this ſtrength of eaſieſt freezing, the addition of more ſnow would produce cold, unleſs this ſtrength be greater than that at which the addition of a ſmall quantity of ſnow begins to produce cold; but even were this the caſe, heat would not be produced, but the temperature of the acids would remain ſtationary until they were ſo much diluted that the addition of more ſnow ſhould produce cold. So that, in either caſe, the heat of the acids, at the time that the addition of freſh ſnow began to produce cold, muſt be that of eaſieſt freezing; and conſequently, as this heat was found to coincide very nearly with the freezing point of theſe acids, after dilution, it follows that their ſtrengths at that time could differ very little from the ſtrength of eaſieſt freezing.

If the temperature of the liquors at the beginning of the experiment had been above the point of eaſieſt freezing, none of the acid would have congealed during the dilution, and nothing could have been learnt from the experiment relating to the point of eaſieſt freezing; but the heat would have kept increaſing, till the acid was diluted to that degree of ſtrength at which the cold produced by the diſſolving of the ſnow was juſt equal to the heat produced by the union of the melted ſnow with the acid*; after which the addition of more ſnow would begin to produce cold. When I recommended this method of finding the beſt ſtrength of ſpirit of nitre for producing cold, by the addition of ſnow, I was not aware of any impediment from the freezing of the acid, in which caſe it would have been a very proper method; but on account of this circumſtance it can hardly be conſidered as ſuch, except when the cold of the acid at ſetting out is leſs than that of eaſieſt freezing.

In the dephlogiſticated ſpirit of nitre the freezing points anſwering to the ſtrength of ,434, ,53, and ,56, were ſaid to be −4°½, −19°, and −30°; and the differences of −30° and −19° from −4°½ are to each other very nearly in the duplicate ratio of ,126 and ,096, the differences of the correſponding ſtrengths from ,434; which, as ,434 is the ſtrength of eaſieſt freezing, is the proportion that might naturally be

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* In the experiment related in my obſervations on Mr. HUTCHINS's Experiments, this ſtrength was rather greater than that of eaſieſt freezing: but whether it is ſo in degrees of cold exceeding that in which my experiment was tried, does not appear.

expected, and conſequently ſerves in ſome meaſure to confirm the reaſoning in this and the 12th Article.

16. After Mr. M^c^ NAB had diluted theſe acids as abovementioned, he divided each of them into two parts, and tried what degree of cold could be produced by mixing them with ſnow. On January 15th, one of theſe parts of the common ſpirit of nitre was tried. It was fluid when the experiment began, though its temperature, as well as that of the ſnow, was −21°½; but on adding ſnow it immediately began to freeze, and grew thick, and its heat increaſed to −2°½; but by the addition of more ſnow it quickly ſunk again, and at laſt got to −43°¼. During the addition of the ſnow, the mixture grew thinner, and by the time it arrived at nearly the greateſt degree of cold, conſiſted viſibly of three parts: the loweſt part, which conſiſted of frozen acid, was white and felt gritty; the upper part, which occupied about an equal ſpace, was alſo white, but felt ſoft, and muſt have conſiſted of unmelted ſnow; the other part, which occupied by much the ſmalleſt ſpace, was clear and fluid. The quantity of ſnow added was about $\frac{2}{11}$ of the weight of the acid, and conſequently its ſtrength was reduced to ,243.

Though ſnow was added to the acid in this experiment as long as, and even longer than, it produced any increaſe of cold, yet ſome days after, on adding more ſnow to the mixture, while it was fluid, and of the temperature of −40°½, the cold was increaſed to −44°¼, or 1 degree lower than before. Mr. M^c^ NAB did not perceive the ſnow to melt, though in all probability ſome muſt have done ſo, or no cold would have been produced.

The cauſe of this ſeems to be, that in the preceding experiment the congealed part of the acid was ſtronger than the fluid part; ſo that, though the fluid part was not ſtrong enough to diſſolve ſnow in a cold greater than − 43°¼, yet the whole acid together was ſtrong enough to do it in a cold one degree greater.

A circumſtance occurred in the laſt experiment which I cannot at all ſee the reaſon of; namely, a ſmall part of the acid being poured into a ſaucer, before the addition of the ſnow, it was in an hour's time changed into ſolid ice, though the cold of the air, at the time the acid was poured out, was only − 41°¼, and does not ſeem to have increaſed during the experiment.

17. On December 30, the other half of the ſame acid had been tried in the ſame manner; at the beginning of the experiment not more than one-ninth part of the acid was fluid, the reſt ſolid clear ice; its temperature was − 34°½, and that of the ſnow nearly the ſame; the greateſt degree of cold produced was − 42°¾; and the quantity of ſnow employed was about one-eighteenth of the weight of the acid; ſo that the ſtrength of the mixture was ,38. The freezing point of the acid thus diluted appears to be about − 45°¼; for by the increaſe of warmth during the day-time, moſt of the congealed matter diſſolved; but in the evening it began to freeze again, ſo as to become thicker, its temperature being then − 45°¼; and the next morning it was frozen ſolid, its cold being one degree greater.

18. On December 12, the diluted ſpirit of nitre N° 139. whoſe ſtrength was ,175, was found frozen, its temperature being − 17. The fluid part, which was full of thin flakes of clear ice, and was of the conſiſtence of ſyrup, was decanted into another bottle, and ſent back. Its ſtrength was ,21, and was greater than that of the undecanted part in the proportion of ,21 to ,16; ſo that, as not much of the undecanted part was

really congealed, the frozen part of the acid muſt have been much weaker than the reſt, if not mere water. Accordingly, during the melting of the undecanted part, the frozen particles ſwam at top. Mr. M^c^ NAB added ſnow to a little of the decanted liquor, but it did not diſſolve, and no increaſe of cold was produced.

19. From theſe experiments it appears, that ſpirit of nitre is ſubject to two kinds of congelation, which we may call the aqueous and ſpirituous; as in the firſt it is chiefly, if not intirely, the watery part which freezes, and in the latter the ſpirit itſelf. Accordingly, when the ſpirit is cooled to the point of aqueous congelation, it has no tendency to diſſolve ſnow and produce cold thereby, but on the contrary is diſpoſed to part with its own water; whereas its tendency to diſſolve ſnow and produce cold, is by no means deſtroyed by being cooled to the point of ſpirituous congelation, or even by being actually congealed. When the acid is exceſſively dilute, the point of aqueous congelation muſt neceſſarily be very little below that of freezing water; when the ſtrength is ,21, it is at −17°, and at the ſtrength of ,243, it ſeems, from Art. 16. to be at −44°½. Spirit of nitre, of the foregoing degrees of ſtrength, is liable only to the aqueous congelation, and it is only in greater ſtrengths that the ſpirituous congelation can take place. This ſeems to be performed with the leaſt degree of cold, when the ſtrength is ,411, in which caſe the freezing point is at −1°½. When the acid is either ſtronger or weaker, it requires a greater degree of cold; and in both caſes the frozen part ſeems to approach nearer to the ſtrength of ,411 than the unfrozen part; it certainly does ſo, when the ſtrength is greater than ,411, and there is little doubt but what it does ſo in the other caſe. At the ſtrength of ,54 the point of ſpirituous

congelation is 31°½, and at ,33 probably −45°½; at leaſt one kind of congelation takes place at that point, and there is little doubt but that it is of the ſpirituous kind. In order to preſent this matter more at one view, I have added the following table of the freezing point of common ſpirit of nitre anſwering to different ſtrengths.

Strength.	Freezing point.	
	°	
,54	−31½	ſpirituous congelation.
,411	−1½*	
,38	−45½	
,243	−44½	aqueous congelation.
,21	−17	

20. In trying the firſt half of the dephlogiſticated ſpirit of nitre, the cold produced was −44°½. The acid was fluid before the addition of the ſnow, and of the temperature of −36°, but froze on putting in the thermometer, and roſe to + 5°, as related in Art. 7.

In trying the ſecond part, the acid was about 0° before the addition of the ſnow, and therefore had no diſpoſition to freeze. The cold produced was −42°½.

As the quantity of ſnow added in theſe experiments was not obſerved, they do not determine any points of aqueous or ſpirituous congelation in this acid; but there is reaſon to think, that theſe points are nearly the ſame as thoſe of common ſpirit of nitre of the ſame ſtrength, as the cold produced in theſe experiments was nearly the ſame as that obtained by the common ſpirit of nitre.

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* The point of eaſieſt freezing.

On

On the Vitriolic Acid.

21. On December 12, the ſtrong oil of vitriol N° 151. was found frozen, and was nearly of the colour and conſiſtence of hogs-lard. Its temperature, found by preſſing the ball of a thermometer into it, was − 15°, and that of the air nearly the ſame; but in the night it had been expoſed to a cold of − 33°. It diſſolved but ſlowly on being brought into a warm room, and was not completely melted before it had riſen to + 20°, and even then was not very fluid, but of a ſyrupy conſiſtence. During the progreſs of the melting, the congealed part ſunk to the bottom, as in ſpirit of nitre; and many air bubbles ſeparated from the acid, which, when it was completely melted, formed a little froth on the ſurface. As ſoon as it was ſufficiently melted to admit of it, which was not till it had riſen to the temperature of + 10°, the fluid part was decanted, and both were ſent home to be examined.

It is remarkable, that the frozen part did not intirely diſſolve until the temperature was ſo much increaſed. This would incline one to think, that the frozen part muſt have differed in ſome reſpect from the reſt, ſo as to require much leſs cold to make it freeze; but yet I could not find that the ſtrength of the decanted part differed ſenſibly from the reſt.

It appeared by another bottle of oil of vitriol, which alſo froze by the natural cold of the air, that this acid, as well as the nitrous, contracts in freezing.

22. On December 21, when the weather was at − 30°, the vitriolic acid N° 103. was diluted with ſnow, as directed in Art. 3. The ſnow diſſolved immediately, and no ſigns of congelation appeared during any part of the proceſs. The

temperature of the acid roſe only one degree before it began to ſink, and the weight of the ſnow added was only $\frac{10}{111}$ of that of the acid, ſo that its ſtrength was reduced thereby to ,605; which is therefore the beſt degree of ſtrength for producing cold by the addition of ſnow, when the degree of cold ſet out with is −30°. This ſtrength is one-fifteenth part leſs than what I found myſelf, by a ſimilar experiment, when the temperature of the acid was +27°; which ſhews, that the beſt degree of ſtrength is rather leſs, when the degree of cold ſet out with is great than when ſmall, but that it does not differ much.

23. The acid thus diluted was divided into two parts, and the next day Mr. M^c Nab tried what degree of cold could be produced by adding ſnow to one of them. The temperature of the air at the time was −39°, and the mixture ſunk by the proceſs to −55°½. The ſnow diſſolved readily, and the mixture did not loſe much of its fluidity until it had acquired nearly its greateſt degree of cold, nor did any congealed matter ſink to the bottom in any part of the proceſs. The quantity of ſnow added was about $\frac{86}{100}$ of the weight of the acid, ſo that the ſtrength of the mixture was about ,325.

24. On January 1, thin cryſtals of ice were found diffuſed all through this mixture, the temperature of the air being −51°½, but that of the liquor was not tried. As this congelation muſt have been of the aqueous kind, and ſeems to have taken place at the temperature of −51°½, it ſhould follow, that this acid had no power of diſſolving ſnow in a cold of 51°½; ſo that it does not at firſt appear why a cold four degrees greater than that ſhould have been produced in the foregoing experiment. The reaſon is, that at the time the mixture arrived at −55°½, it appeared by the diminution of its fluidity to have

contained ſome undiſſolved ſnow, and ſome more was added to it after that time, which before the firſt of January diſſolved and mixed with the acid; ſo that the acid in the mixture, at the time it ſunk to − 55°½, was not quite ſo much diluted as that which froze on January 1. This is the reverſe of what happened in the trial of the nitrous acid in Art. 15. as in that experiment the fluid part, at the time of the greateſt cold, was weaker than the whole mixture together; but it muſt be conſidered, that *that* mixture contained much congealed acid, as well as undiſſolved ſnow, whereas *this* contained only the latter.

25. On January 1, ſnow was added to the other half of the acid diluted on December 21. The cold produced was much greater than before, namely − 68°½; this ſeems to have proceeded, partly from the air and materials having been 12 degrees colder in this than in the former experiment, and partly from the ſnow having been added faſter, ſo that the mixture arrived at its greateſt degree of cold in 20′, whereas it before took up 46′. Another reaſon is, that the former mixture was made in too ſmall a jar, in conſequence of which it was poured into a larger before the experiment was completed, whereby ſome cold was loſt. The quantity of ſnow uſed in this experiment was leſs than in the former, ſo that the ſtrength of the acid after the experiment was about ,343. The mixture alſo grew much thicker, and had a degree of elaſticity reſembling jelly; but whether this was owing only to more ſnow remaining undiſſolved, or to any other cauſe, I cannot tell.

26. Great as the foregoing degree of cold is, Mr. M^c^ Nab, on February 2, produced one much greater. In hopes of obtaining a greater degree of cold by previouſly cooling the materials, he cooled about ſeven ounces of oil of vitriol, whoſe ſtrength was ,629, that is, rather ſtronger than the foregoing,

by placing the jar in which it was contained in a freezing mixture of oil of vitriol and ſnow; the ſnow intended to be uſed was alſo cooled by placing it under the veſſel in which the freezing mixture was made. As ſoon as the acid in the jar was cooled to the temperature of −57°½, a little of the ſnow was added, on which it immediately began to freeze, and roſe to −36°; but in about 40 minutes, as the jar was ſtill kept in the freezing mixture, it ſunk to −48°; by which time it was grown very thick and gritty, eſpecially at bottom. More of the cooled ſnow was then added, which in a ſhort time made it ſink to −78°½, and at the ſame time the thickneſs and tenacity of the mixture diminiſhed; ſo that by the time it arrived at the greateſt degree of cold, very little thickneſs remained.

It is worth inquiring, what was the reaſon of the greater degree of cold produced in this than in the preceding experiment? It could not be owing to the materials being colder; for at the time of the ſecond addition of ſnow, at which time the experiment may be conſidered to have begun, the acid was not colder than at the beginning of the preceding experiment, and the ſnow in all probability not much colder. It could not be owing neither to the jar having been kept in the freezing mixture: for though that mixture was three or four degrees colder than the air in the preceding experiment, yet the acid in the jar, before it acquired much addition of cold, would be robbed of its cold faſter by the mixture than it would by air of the ſame temperature as that in the preceding experiment. Neither could it proceed from any difference in the ſtrength of the acid; for what difference there was muſt have done more hurt than good. The true reaſon is, that the acid was in a ſtate of congelation: for as the congealed acid united to the ſnow and became fluid by the union, it is plain, that cold muſt have been produced both by the melting of the ſnow and by that of the acid; whereas, if the acid had been in a fluid ſtate, cold would have been produced only by the firſt cauſe, and conſequently a greater degree of cold ſhould be produced in this experiment than in the former. The only inconvenience attending the acid being in a ſtate of congelation is, that in all probability it does not unite to the ſnow ſo readily as when in a fluid ſtate; but the difference ſeems not material, as the cold was produced, and the materials melted, in 5 minutes.

27. The day before, Mr. Mc Nab, by adding ſnow to ſome of the ſame acid in the uſual manner, when the cold of the materials was −46°, produced a cold of only −66°.

28. In theſe four laſt experiments the acid was reduced, by the addition of the ſnow, to the ſtrengths of ,325, ,343, ,403, and ,334; and the cold produced in them was before ſaid to be −55°½, −68°½, −78°½, and −66°; whence we may conclude, that theſe are nearly the points of aqueous congelation anſwering to the foregoing ſtrengths; only it appears, from what was ſaid in Art. 24. that the ſtrengths here ſet down are all of them rather too ſmall.

Though it is certain that oil of vitriol is capable of the ſpirituous congelation, and though it appears, both from the foregoing experiments and from ſome made by the Duc D'AYEN * and by M. DE MORVEAU †, that it freezes with a leſs degree of cold when ſtrong than when much diluted, it is not certain whether it has any point of eaſieſt freezing, like ſpirit of nitre, or whether the cold required to freeze it does not continually diminiſh as the ſtrength increaſes, without limitation; but the latter opinion is the moſt probable. For the Duc D'AYEN's and

* Diction. de Chym. par MACQUER, 2de edit.

† Nouv. Mém. de l'Académ. de Dijon, 1782, 1er ſemeſtre, p. 68.

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M. DE MORVEAU's acids, which, as they were concentrated on purpoſe, were moſt likely ſtronger than Mr. M^c NAB's, froze with a cold leſs than zero of FAHRENHEIT; whereas the freezing point of Mr. M^c NAB's undiluted acid, whoſe ſtrength was ,98, was − 15°, and that of the diluted acid, whoſe ſtrength was ,629, was − 36°; and when the acid was more diluted, it was found to bear a much greater cold without freezing. It appears alſo, both from Art. 21. and from M. DE MORVEAU's experiment, that during the congelation of the oil of vitriol, ſome ſeparation of its parts takes place, ſo that the congealed part differs in ſome reſpect from the reſt, in conſequence of which it freezes with a leſs degree of cold; and as there is reaſon to think from Art. 21. that theſe two parts do not differ much in ſtrength, it ſeems as if the difference between them depended on ſome leſs obvious quality, and probably on that, whatever it is, which forms the difference between glacial and common oil of vitriol. The oil of vitriol prepared from green vitriol, has ſometimes been obtained in ſuch a ſtate as to remain conſtantly congealed, except when expoſed to a heat conſiderably greater than that of the atmoſphere, whence it acquired its name of *glacial* *. It is not known indeed upon what this property depends, but it is certainly ſomething elſe than its ſtrength; for oil of vitriol of this kind is always ſmoking, and the fumes it emits are particularly oppreſſive and ſuffocating, though very different from thoſe of the volatile ſulphureous acid. On rectification likewiſe it yields, with the gentleſt heat, a peculiar concrete ſubſtance, in the form of ſaline cryſtals; and after this volatile part has been driven off, the remainder is no longer ſmoking, and has loſt its glacial quality *.

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* Mém. de l'Académ. des Sc. 1738, p. 288.

On the Mixture of Oil of Vitriol and Spirit of Nitre.

29. This mixture is not ſo fit for producing cold by the addition of ſnow, as oil of vitriol alone; for the cold obtained did not exceed − 54°½, in either of the experiments tried with it. The point of ſpirituous congelation of this mixture, when diluted with ſomewhat more than one-tenth of its weight of water, is about − 20°, and is much lower when the acid is conſiderably more diluted: but as the Society will moſt likely have leſs curioſity about the diſpoſition to freeze of this mixture than of the ſimple acids, I ſhall ſpare the particulars.

On the Spirit of Wine.

30. The rectified ſpirits N° 8. were diluted with ſnow, in the ſame manner as the other liquors; but were found not to want any, as the firſt and only addition of ſnow produced cold. The quantity added was about $\frac{1}{26}$ of the weight of the ſpirit.

31. The ſpirit thus diluted was divided, like the other liquors, into two parts, and each tried ſeparately. The firſt was at − 45°, before the addition of the ſnow, and was ſunk by the proceſs to − 56°. The ſnow, even at the firſt addition, did not diſſolve well, ſo that the ſpirit immediately

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* Crell's Neu. Entdeck. in der Chemie, Th. 11. p. 100. Th. 12. p. 241, &c. and Annalen, 1785, St. 5. p. 438, &c.

became full of white ſpots *, and grew thick by the time it arrived at its greateſt degree of cold. After ſtanding ſome hours, the mixture roſe to the temperature of − 39°, and was grown clear, but yet was not limpid, but of the conſiſtence of ſyrup. No cold was produced by adding ſnow to it in that ſtate, though it appeared that its point of aqueous congelation was at leaſt 6 degrees lower than its temperature at that time †; which ſeems to ſhew that ſpirit of wine has ſcarce any power of diſſolving ſnow when it wants even 6 degrees of its point of aqueous congelation, and therefore is another inſtance that ſnow is diſſolved much leſs readily by ſpirit of wine than by the nitrous and vitriolic acids.

32. In trying the other part of the diluted ſpirits, the cold produced was only − 47°½, the cold ſet out with being − 37°.

33. It appeared by the diluted ſpirit of wine N° 143. which on December 12 froze by the natural cold of the atmoſphere, and was treated in the ſame manner as the diluted ſpirit of nitre, that when highly rectified ſpirit of wine, ſuch as N° 8. is diluted with 1 4/10 its weight of water, its point of aqueous congelation will be at − 21°. The congealed part of the ſpirit was white like diluted milk, and even the decanted part, which was full of thin films of ice, had a milky hue. The fluid part was ſtronger than the reſt, and no increaſe of cold was produced by adding ſnow to ſome of it, both of which are marks of aqueous congelation.

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* This was not the caſe during the above-mentioned dilution of the ſpirits; but the cold was 16 degrees leſs in that experiment than in this.

† On account of the dilution which the ſpirits ſuffered by the melting of the ſnow which remained undiſſolved at the time of the greateſt cold, its point of aqueous congelation was no longer ſo low as − 56°; but it ſtill was not leſs than − 48½, as in the evening it was found at that temperature, without much congealed matter in it.

Though the foregoing experiments confirm the truth of what I ſaid, in the account of Mr. HUTCHINS's experiments, concerning the cauſe of the cold produced by mixing ſnow with different liquors, and intirely clear up the difficulty relating to it which I mentioned in Art. 1. yet ſeveral queſtions may naturally occur; ſuch as, why the cold produced by the oil of vitriol was ſo much greater than that obtained by the ſpirit of nitre, notwithſtanding that in warmer climates the nitrous acid ſeems to produce more cold? and why the cold produced by the nitrous acid, notwithſtanding its previous dilution, which might naturally be expected to be of ſervice, was not greater than has been obtained by other perſons without that precaution? But as this would lead me into diſquiſitions of conſiderable length, without my being able to ſay any thing very ſatisfactory on the ſubject, I ſhall forbear entering into it. I will only obſerve, that in moſt of the foregoing experiments, Mr. M^c NAB would probably have produced more cold, if he had added the ſnow faſter. We ought not, however, to regret that he did not, as its effects on the acids would then have been leſs ſenſible.

The natural cold, when theſe experiments were made, is remarkable; as there were at leaſt nine mornings in which the cold was not leſs than that of freezing mercury; four in which it was at leaſt eight degrees below that point, or −47°; and one in which it was −50°. Whereas out of nine winters, during which Mr. HUTCHINS obſerved the thermometer at Albany Fort, there were only twelve days in which the cold was equal to that of freezing mercury, and the greateſt cold ſeems to have been −45°. I cannot learn whether the laſt winter was more ſevere than uſual at Hudſon's Bay; or whether Henley-Houſe is a colder ſituation than Albany, which may perhaps be the caſe; for though it is only 130 miles diſtant from it, yet it ſtands inland, and to the W. or S.W. of it, which is the quarter from which the coldeſt winds blow.

Mr. Mc NAB's original account of the experiments which furniſhed the materials of this Paper, having been thought too long to be printed in detail, is depoſited in the Archives of the Society.

END OF PART I. OF VOL. LXXVI.

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PHILOSOPHICAL TRANSACTIONS.

XIV. *New Experiments upon Heat. By Colonel Sir* Benjamin Thompſon, *Knt. F. R. S. In a Letter to Sir* Joſeph Banks, *Bart. P. R. S.*

Read March 9, 1786.

DEAR SIR,

I HAVE at length begun the courſe of experiments upon heat which I have ſo long had in contemplation; and I have already made a diſcovery, which, if not new to you, is perfectly ſo to me, and which I think may lead to a further knowledge reſpecting the nature of heat.

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Examining the conducting power of air, and of various other fluid and ſolid bodies, with regard to heat, I was led to examine the conducting power of the *Torricellian vacuum*. From the ſtriking analogy between the electric fluid and heat reſpecting their conductors and non-conductors (having found that bodies, in general, which are conductors of the electric fluid, are likewiſe good conductors of heat, and, on the contrary, that electric bodies, or ſuch as are bad conductors of the electric fluid, are likewiſe bad conductors of heat), I was led to imagine that the Torricellian vacuum, which is known to afford ſo ready a paſſage to the electric fluid, would alſo have afforded a ready paſſage to heat.

The common experiments of heating and cooling bodies under the receiver of an air-pump I concluded inadequate to determining this queſtion; ~~not only on~~ account of the impoſſibility of making a perfect void of air by means of the pump; but alſo on account of the moiſt vapour, which exhaling from the wet leather and the oil uſed in the machine, expands under the receiver, and fills it with a watery fluid, which, though extremely rare, is yet capable of conducting a great deal of heat: I had recourſe therefore to other contrivances.

I took a thermometer, unfilled, the diameter of whoſe bulb (which was globular) was juſt half an inch, Paris meaſure, and fixed it in the center of a hollow glaſs ball of the diameter of 1½ Paris inch, in ſuch a manner, that the ſhort neck or opening of the ball being ſoldered faſt to the tube of the thermometer 7½ lines above its bulb, the bulb of the thermometer remained fixed in the center of the ball, and conſequently was cut off from all communication with the external air. In the bottom of the glaſs ball was fixed a ſmall hollow tube or point, which projecting outwards was ſoldered to the end of a common barometer tube about 32 inches in length, and by means of this opening the ſpace between the internal ſurface of the glaſs ball and the bulb of the thermometer was filled with hot mercury, which had been previouſly freed of air and moiſture by boiling. The ball, and alſo the barometrical tube attached to it, being filled with mercury, the tube was carefully inverted, and its open end placed in a bowl in which there was a quantity of mercury. The inſtrument now became a barometer, and the mercury deſcending from the ball (which was now uppermoſt) left the ſpace ſurrounding the bulb of the thermometer free of air. The mercury having totally quitted the glaſs ball, and having ſunk in the tube to the height of 28 inches (being the height of the mercury in the common barometer at that time), with a lamp and a blow-pipe I melted the tube together, or ſealed it hermetically, about three-quarters of an inch below the ball, and cutting it at this place with a fine file, I ſeparated the ball from the long barometrical tube. The thermometer being afterwards filled with mercury in the common way, I now poſſeſſed a thermometer whoſe bulb was confined in the center of a *Torricellian vacuum*, and which ſerved at the ſame time as the body to be heated, and as the inſtrument for meaſuring the heat communicated.

Experiment N° 1.

With this inſtrument (ſee fig. 1. Tab. VI.) I made the following experiment. Having plunged it into a veſſel filled with water, warm to the 18th degree of REAUMUR's ſcale, and ſuffered it to remain there till it had acquired the temperature of the water, that is to ſay, till the mercury in the incloſed thermometer ſtood at 18°, I took it out of this veſſel and plunged it ſuddenly into a veſſel of boiling water, and holding it in the water (which was kept conſtantly boiling) by the end of the tube, in ſuch a manner that the glaſs ball, in the center of which was the bulb of the thermometer, was juſt ſubmerged, I obſerved the number of degrees to which the mercury in the thermometer had ariſen at different periods of time, counted from the moment of its immerſion. Thus, after it had remained in the boiling water 1 min. 30 ſec. I found the mercury had riſen from 18° to 27°. After 4 minutes had elapſed, it had riſen to $44^{\circ}\frac{9}{10}$; and at the end of 5 minutes it had riſen to $48^{\circ}\frac{2}{10}$.

Experiment N° 2.

Taking it now out of the boiling water I ſuffered it to cool gradually in the air, and after it had acquired the temperature of the atmoſphere, which was that of 15° R. (the weather being perfectly fine), I broke off a little piece from the point of the ſmall tube which remained at the bottom of the glaſs ball, where it had been hermetically ſealed, and of courſe the atmoſpheric air ruſhed immediately into the ball. The ball ſurrounding the bulb of the thermometer being now filled with air (inſtead of being emptied of air, as it was in the before-mentioned experiment), I re-ſealed the end of the ſmall tube at the bottom of the glaſs ball hermetically, and by that means cut off all communication between the air confined in the ball and the external air; and with the inſtrument ſo prepared I repeated the experiment before-mentioned; that is to ſay, I put it into water warmed to 18°, and when it had acquired the temperature

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perature of the water, I plunged it into boiling water, and obſerved the times of the aſcent of the mercury in the thermometer. They were as follows:

	Time elapſed.	Heat acquired.
Heat at the moment of being plunged into the boiling water, . . .		18° R.
	M. S.	°
After having remained in the boiling water	0 45	27
	1 0	$34\frac{4}{10}$
	2 10	$44\frac{9}{10}$
	2 40	$48\frac{2}{10}$
	4 0	$56\frac{3}{10}$
	5 0	$60\frac{9}{10}$

From the reſult of theſe experiments it appears evidently, that the Torricellian vacuum, which affords ſo ready a paſſage to the electric fluid, ſo far from being a good conductor of heat, is a much worſe one than common air, which of itſelf is reckoned among the worſt: for in the laſt experiment, when the bulb of the thermometer was ſurrounded with air, and the inſtrument was plunged into boiling water, the mercury roſe from 18° to 27° in 45 ſeconds; but in the former experiment, when it was ſurrounded by a Torricellian vacuum, it required to remain in the boiling water 1 minute 30 ſeconds = 90 ſeconds, to acquire that degree of heat. In the vacuum it required 5 minutes to riſe to $48^{\circ}\frac{2}{10}$; but in air it roſe to that height in 2 minutes 40 ſeconds; and the proportion of the times in the other obſervations is nearly the ſame, as will appear by the following table.

The

	The bulb of the thermometer placed in the center of the glaſs ball, and			
	ſurrounded by a Torricellian vacuum. (Exp. N° 1.)		ſurrounded by air. (Exp. N° 2.)	
	Time elapſed.	Heat acquired.	Time elapſed.	Heat acquired.
Upon being plunged into boiling water		18°		18°
	M. S.	°	M. S.	°
After remaining in it	1 30	27	0 45	27
	——	——	1 0	$30\frac{4}{10}$
	4 0	$44\frac{9}{10}$	2 10	$44\frac{9}{10}$
	5 0	$48\frac{2}{10}$	2 40	$48\frac{2}{10}$
	——	——	4 0	$56\frac{2}{10}$
	——	——	5 0	$60\frac{9}{10}$

Theſe experiments were made at Manheim, upon the firſt day of July laſt, in the preſence of Profeſſor Hemmer, of the Electoral Academy of Sciences of Manheim, and Charles Artaria, Meteorological Inſtrument-maker to the Academy, by whom I was aſſiſted.

Finding the conſtruction of the inſtrument made uſe of in theſe experiments attended with much trouble and riſque, on account of the difficulty of ſoldering the glaſs ball to the tube of the thermometer without at the ſame time either cloſing up, or otherwiſe injuring, the bore of the tube, I had recourſe to another contrivance much more commodious, and much eaſier in the execution.

At the end of a glaſs tube or cylinder ten or eleven inches in length, and near three-quarters of an inch in diameter internally, I cauſed a hollow globe to be blown 1½ inch in diameter,

meter, with an opening in the bottom of it correſponding with the bore of the tube, and equal to it in diameter, leaving to the opening a neck or ſhort tube, about an inch or three-quarters of an inch in length. Having a thermometer prepared, whoſe bulb was juſt half an inch in diameter, and whoſe freezing point fell at about 2¾ inches above its bulb, I graduated its tube according to REAUMUR's ſcale, beginning at 0°, and marking that point, and alſo every tenth degree above it to 80°, with threads of fine ſilk bound round it, which being moiſtened with lac varniſh adhered firmly to the tube. This thermometer I introduced into the glaſs cylinder and globe juſt deſcribed, by the opening in the bottom of the globe, having firſt choaked the cylinder at about 2 inches from its junction with the globe by heating it, and crowding its ſides inwards towards its axis, leaving only an opening ſufficient to admit the tube of the thermometer. The thermometer being introduced into the cylinder in ſuch a manner that the center of its bulb coincided with the center of the globe, I marked a place in the cylinder, about three-quarters of an inch above the 80th degree or boiling point upon the tube of the incloſed thermometer, and taking out the thermometer, I choaked the cylinder again in this place. Introducing now the thermometer for the laſt time, I cloſed the opening at the bottom of the globe at the lamp, taking care, before I brought it to the fire, to turn the cylinder upſide down, and to let the bulb of the thermometer fall into the cylinder till it reſted upon the lower choak in the cylinder. By this means the bulb of the thermometer was removed more than 3 inches from the flame of the lamp. The opening at the bottom of the globe being now cloſed, and the bulb of the thermometer being ſuffered to return into the globe, the end of the cylinder was cut off to within about half an inch of the upper choak. This being done, it is plain, that the tube of the thermometer projected beyond the end of the cylinder. Taking hold of the end of the tube, I placed the bulb of the thermometer as nearly as poſſible in the center of the globe, and obſerving and marking a point in the tube immediately above the upper choak of the cylinder, I turned the cylinder upſide down, and ſuffering the bulb of the thermometer to enter the cylinder, and reſt upon the firſt or lower choak (by which means the end of the tube of the thermometer came further out of the cylinder), the end of the tube was cut off at the mark juſt mentioned (having firſt taken care to melt the internal cavity or bore of the tube together at that place), and a ſmall ſolid ball of glaſs, a little larger than the internal diameter or opening of the choak, was ſoldered to the end of the tube, forming a little button or knob, which reſting upon the upper choak of the cylinder, ſerved to ſuſpend the thermometer in ſuch a manner that the center of its bulb coincided with the center of the globe in which it was ſhut up. The end of the cylinder above the upper choak being now heated and drawn out to a point; or rather being formed into the figure of the fruſtum of a hollow cone, the end of it was ſoldered to the end of a barometrical tube, by the help of which the cavity of the cylinder and globe containing the thermometer was completely voided of air with mercury; when, the end of the cylinder being hermetically ſealed, the barometrical tube was detached from it with a file, and the thermometer was left completely ſhut up in a Torricellian vacuum, the center of the bulb of the thermometer being confined in the center of the glaſs globe, without touching it in any part, by means of the two choaks in the cylinder, and the button upon the end of the tube.

Of theſe inſtruments I provided myſelf with two, as nearly as poſſible of the ſame dimenſions; the one, which I ſhall call N° 1. being voided of air, in the manner above deſcribed; the other, N° 2. being filled with air, and hermetically ſealed.

With theſe two inſtruments (ſee fig. 2.) I made the following experiments upon the 11th of July laſt, at Manheim, between the hours of ten and twelve, the weather being very fine and clear, the mercury in the barometer ſtanding at 27 inches 11 lines, Reaumur's thermometer at 15°, and the quill hygrometer of the Academy of Manheim at 47°.

Experiments N° 3, 4, 5 and 6.

Putting both the inſtruments into melting ice, I let them remain there till the mercury in the incloſed thermometers reſted at the point 0°, that is to ſay, till they had acquired exactly the temperature of freezing water or melting ice; and then taking them out of the ice I plunged them ſuddenly into a large veſſel of boiling water, and obſerved the time required for the mercury to riſe in the thermometers from ten degrees to ten degrees, from 0° to 80°, taking care to keep the water conſtantly boiling during the whole of this time, and taking care alſo to keep the inſtruments immerſed to the ſame depth, that is to ſay, juſt ſo deep that the point 0° of the incloſed thermometer was even with the ſurface of the water.

Theſe experiments I repeated twice, with the utmoſt care; and the following table gives the reſult of them.

Thermometer N° 1.

Its bulb half an inch in diameter, ſhut up in the center of a hollow glaſs globe, 1½ inch in diameter, *void of air*, and hermetically ſealed.

Taken out of freezing water, and plunged into boiling water.

Time elapſed.				Heat acquired.
Exp. N° 3.		Exp. N° 4.		
M.	S.	M.	S.	
				0°
0	51	0	51	10
0	59	0	59	20
1	1	1	2	30
1	18	1	22	40
1	24	1	23	50
2	0	1	51	60
3	30	3	6	70
11	41	10	27	80
22	44	21	1	= total time of heating from 0° to 80°.

Total time from 0° to 70°:

	M.	S.
In Exp. N° 3. =	11	3
In Exp. N° 4. =	10	34
Medium =	10	48½

Thermometer N° 2.

Its bulb half an inch in diameter, ſhut up in the center of a hollow glaſs globe, 1½ inch in diameter, *filled with air*, and hermetically ſealed.

Taken out of freezing water, and plunged into boiling water.

Time elapſed.				Heat acquired.
Exp. N° 5.		Exp. N° 6.		
M.	S.	M.	S.	
				0°
0	30	0	30	10
0	35	0	37	20
0	41	0	41	30
0	49	0	53	40
1	1	0	59	50
1	24	1	20	60
2	45	2	25	70
9	10	9	38	80
16	55	17	3	= total time of heating from 0° to 80°.

Total time from 0° to 70°:

	M.	S.
In Exp. N° 5. =	7	45
In Exp. N° 6. =	7	25
Medium =	7	35

It appears from theſe experiments, that the conducting power of air to that of the Torricellian vacuum, under the circumſtances deſcribed, is as $7\frac{35}{60}$ to $10\frac{48\frac{1}{2}}{60}$ inverſely, or as 1000 to 702 nearly; for the quantities of heat communicated being equal, the intenſity of the communication is as the times inverſely.

In theſe experiments the heat paſſed through the ſurrounding medium *into* the bulb of the thermometer: in order to reverſe the experiment, and make the heat paſs *out of* the thermometer, I put the inſtruments into boiling water, and let them remain therein till they had acquired the temperature of the water, that is to ſay, till the mercury in the incloſed thermometers ſtood at 80°; and then, taking them out of the boiling water, I plunged them ſuddenly into a mixture of water and pounded ice, and moving them about continually in this mixture, I obſerved the times employed in cooling as follows.

Thermometer N° 1.
Surrounded by a *Torricellian vacuum.*
Taken out of boiling water, and plunged into freezing water.

Time elapſed.		Heat loſt.
Exp. N° 7.	Exp. N° 8.	
		80°
M. S.	M. S.	°
1 2	0 54	70
0 58	1 2	60
1 17	1 18	50
1 46	1 37	40
2 5	2 16	30
3 14	3 10	20
5 42	5 59	10
Not obſerved.	Not obſerved.	0

Total time of cooling from 80° to 10°.
M. S.
In Exp. N° 7. = 16 4
In Exp. N° 8. = 16 16
Medium = 16 10

Thermometer N° 2.
Surrounded by *air.*
Taken out of boiling water, and plunged into freezing water.

Time elapſed.		Heat loſt.
Exp. N° 9.	Exp. N° 10.	
		80°
M. S.	M. S.	
0 33	0 33	70
0 39	0 34	60
0 44	0 44	50
0 55	0 55	40
1 17	1 18	30
1 57	1 57	20
3 44	3 40	10
40 10	Not obſerved.	0

Total time of cooling from 80° to 10°.
M. S.
In Exp. N° 9. = 9 49
In Exp. N° 10. = 9 41
Medium = 9 45

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By theſe experiments it appears, that the conducting power of air is to that of the Torricellian vacuum as $9\frac{45}{60}$ to $16\frac{10}{60}$ inverſely, or as 1000 to 603.

To determine whether the ſame law would hold good when the heated thermometers, inſtead of being plunged into freezing water, were ſuffered to cool in the open air, I made the following experiments. The thermometers N° 1. and N° 2. being again heated in boiling water, as in the laſt experiments, I took them out of the water, and ſuſpended them in the middle of a large room, where the air (which appeared to be perfectly at reſt, the windows and doors being all ſhut) was warm to the 16th degree of REAUMUR'S thermometer, and the times of cooling were obſerved as follows.

(Exp. N° 11.) *Thermometer* N° 1. Surrounded by a Torricellian vacuum. *Heated to* 80°, *and ſuſpended in the open air warm to* 16°.		(Exp. N° 12.) *Thermometer* N° 2. Surrounded by air. *Heated to* 80°, *and ſuſpended in the open air warm to* 16°.	
Time elapſed.	Heat loſt.	Time elapſed.	Heat loſt.
	80°		80°
M. S.	°	M. S.	°
Not obſerved.	70	Not obſerved.	70
1 24	60	0 51	60
1 44	50	1 5	50
2 28	40	1 34	40
4 16	30	2 41	30
10 12 = total time employed in cooling from 70° to 30°.		6 11 = total time employed in cooling from 70° to 30°.	

Here the difference in the conducting powers of air and of the Torricellian vacuum appears to be nearly the ſame as in the foregoing experiments, being as $6\frac{11}{60}$ to $10\frac{12}{60}$ inverſely, or as 1000 to 605. I could not obſerve the time of cooling from

80° to 70°, being at that time buſied in ſuſpending the inſtruments.

As it might poſſibly be objected to the concluſions drawn from theſe experiments that, notwithſtanding all the care that was taken in the conſtructing of the two inſtruments made uſe of that they ſhould be perfectly alike, yet they might in reality be ſo far different, either in ſhape or ſize, as to occaſion a very ſenſible error in the reſult of the experiments; to remove theſe doubts I made the following experiments.

In the morning towards eleven o'clock, the weather being remarkably fine, the mercury in the barometer ſtanding at 27 inches 11 lines, REAUMUR's thermometer at 15°, and the hygrometer at 47°, I repeated the experiment N° 3. (of heating the thermometer N° 1. in boiling water, &c.) and immediately afterwards opening the cylinder containing the thermometer at its upper end, where it had been ſealed, and letting the air into it, I re-ſealed it hermetically, and repeated the experiment again with the ſame inſtrument, the thermometer being now ſurrounded with air, like the thermometer N° 2.

The reſult of theſe experiments, which may be ſeen in the following table, ſhews evidently, that the error ariſing from the difference of the ſhapes or dimenſions of the two inſtruments in queſtion was inconſiderable, if not totally imperceptible.

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(Exp. N° 13.)

Thermometer N° 1.

Its bulb half an inch in diameter ſhut up in the center of a glaſs globe 1½ inch in diameter, *voided of air*, and hermetically ſealed.

Taken out of freezing water, and plunged into boiling water.

Time elapſed.		Heat acquired.
		0°
M.	S.	°
0	55	10
0	55	20
1	7	30
1	15	40
1	29	50
2	2	60
3	21	70
13	44	80

24 48 = total time of heating from 0° to 80°.

Total time from 0° to 70° = 11′ 4″.

(Exp. N° 14.)

The ſame Thermometer (N° 1.)

The glaſs globe, containing the bulb of the thermometer, being now *filled with air*, and hermetically ſealed.

Taken out of freezing water, and plunged into boiling water.

Time elapſed.		Heat acquired.
		0°
M.	S.	°
0	32	10
0	32	20
0	43	30
0	50	40
1	1	50
1	24	60
2	38	70
10	25	80

18 5 = total time of heating from 0° to 80°.

Total time from 0° to 70° = 7′ 40″.

It appears, therefore, from theſe experiments, that the conducting power of common atmoſpheric air is to that of the Torricellian vacuum as $7\frac{40}{60}$ to $11\frac{4}{60}$ inverſely, or as 1000 to 602; which differs but very little from the reſult of all the foregoing experiments.

Notwithſtanding that it appeared, from the reſult of theſe laſt experiments, that any difference there might poſſibly have been in the proportions or dimenſions of the inſtruments N° 1. and N° 2. could hardly have produced any ſenſible error in the reſult of the experiments in queſtion; I was willing, however, to ſee how far any conſiderable alterations of ſize in the inſtrument would affect the experiment: I therefore provided myſelf with another inſtrument, which I ſhall call *Thermometer* N° 3. different from thoſe already deſcribed in ſize, and a little different in its conſtruction.

The bulb of the thermometer was of the ſame form and ſize as in the inſtruments N° 1. and N° 2. that is to ſay, it was globular; and half an inch in diameter; but the glaſs globe, in the center of which it was confined, was much larger, being 3 inches 7½ lines in diameter; and the bore of the tube of the thermometer was much finer, and conſequently its length, and the diviſions of its ſcale, were greater. The diviſions were marked upon the tube with threads of ſilk of different colours at every tenth degree, from 0° to 80°, as in the before-mentioned inſtruments. The tube or cylinder belonging to the glaſs globe was 8 lines in diameter, a little longer than the tube of the thermometer, and perfectly cylindrical from its upper end to its junction with the globe, being without any choak; the thermometer being confined in the center of the globe by a different contrivance, which was as follows. To the opening of the cylinder was fitted a ſtopple of dry wood, covered with a coat of hard varniſh, through the center or axis of which paſſed the end of the tube of the thermometer: this confined the tube in the axis of the cylinder at its upper end. To confine it at its lower end, there was fitted to it a ſmall ſteel ſpring, a little below the point 0°; which, being confined round the tube of the thermometer, had three elaſtic points projecting outwards, which preſſing againſt the inſide of the cylinder, confined the thermometer in its place. The total length of this inſtrument, from the bottom of the globe to the upper end of the cylinder, was 18 inches, and the freezing point upon the thermometer fell about 3 inches above the bulb; conſequently it lay about 1½ inch above the junction of the cylinder with the globe, when the thermometer was confined in its place, the center of its bulb coinciding with the center of the globe. Through the ſtopple which cloſed the end of the cylinder paſſed two ſmall glaſs tubes, about a line in diameter, which being about a line longer than the ſtopple were ſtopped up occaſionally with ſmall ſtopples fitted to their bores. Theſe tubes (which were fitted exactly in the holes bored in the great ſtopple of the cylinder to receive them, and fixed in their places with cement) ſerved to convey air, or any other fluid, into the glaſs ball, without being under a neceſſity of removing the ſtopple cloſing the end of the cylinder; which, in order to prevent the poſition of the thermometer from being eaſily deranged, was cemented in its place.

I have been the more particular in the deſcription of theſe inſtruments, as I conceive it abſolutely neceſſary to have a perfect idea of them in order to judge of the experiments made with them.

With the inſtrument laſt deſcribed (which I have called *Thermometer* N° 3.) I made the following experiment. It was upon the 18th of July laſt, in the afternoon, the weather variable, alternate clouds and ſun-ſhine; wind ſtrong at S.E. with now and then a ſprinkling of rain; barometer at 27 inches 10½ lines, thermometer at 18°¼, and hygrometer variable from 44° to extreme moiſture.

In order to compare the reſult of the experiment made with this thermometer with thoſe made with the thermometer N° 2. I have, in the following table, placed theſe experiments by the ſide of each other.

(Exp. N° 15.)

Thermometer N° 3.

Its bulb half an inch in diameter, ſhut up in the center of a glaſs tube, 3 inches 7½ lines in diameter, and ſurrounded by *air.*

Taken out of freezing water, and plunged into boiling water.

Time elapſed.	Heat acquired.
M. S.	°
0 33	10
0 38	20
0 54	30
0 51	40
1 7	50
1 28	60
2 28	70
9 0	80

16 59 = total time of heating from 0° to 80°.

Time from 0° to 70° = 7′ 59″.

(Exp. N° 4. and N° 5.)

Thermometer N° 2.

Its bulb half an inch in diameter, ſhut up in the center of a glaſs globe, 1½ inch in diameter, and ſurrounded by *air.*

Taken out of freezing water, and plunged into boiling water.

Time elapſed.			Heat acquired.
Exp. N° 4.	Exp. N° 5.	Medium.	°
M. S.	M. S.	M. S.	°
0 30	0 30	0 30	10
0 35	0 37	0 36	20
0 41	0 41	0 41	30
0 49	0 53	0 51	40
1 1	0 59	1 0	50
1 24	1 20	1 22	60
2 45	2 25	2 35	70
9 10	9 38	9 24	80
16 55	17 3	16 59	= total

time of heating from 0° to 80°.

Time from 0° to 70° = 7′ 35″.

If the agreement of theſe experiments with the thermometers N° 2. and N° 3. ſurpriſed me, I was not leſs ſurpriſed with their diſagreement in the experiment which follows.

Experiment N° 16.

Taking the thermometer N° 3. out of the boiling water, I immediately ſuſpended it in the middle of a large room, where the air, which was quiet, had the temperature of 18° ½ R. and obſerved the times of cooling as follows:

Time elapſed.		Heat loſt.
		80°
M.	S.	°
1	55	70
0	12	60
0	33	50
2	15	40
4	0	30

9' 55 = total time of cooling from 80° to 30°.

Time from 70° to 30° = 8' 0''; but in the experiment N° 12. with the thermometer N° 2. the time employed in cooling from 70° to 30° was only 6' 11''. In this experiment, with the thermometer N° 3. the time employed in cooling from 60° to 50° was 7' 48''; but in the above-mentioned experiment, with the thermometer N° 2. it was only 5' 20''. It is true, the air of the room was ſomewhat cooler when the former experiment was made than when this latter was made with the thermometer N° 3.; but this difference of temperature, which was only 2°¼ (in the former caſe the thermometer in the room ſtanding at 16°, and in the latter at 18°¼) certainly could not have occaſioned the whole of the apparent difference in the reſults of the experiments.

Does air receive heat more readily than it parts with it? This is a queſtion highly deſerving of further inveſtigation, and I ſhall not fail to give it a full examination in the courſe of my projected inquiries; but leaving it for the preſent, I ſhall proceed to give an account of the experiments which I have already made *.

It

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* Conceiving it to be a ſtep of conſiderable importance towards coming at a further

It having been my intention from the beginning to examine the conducting powers of the artificial airs or gaſſes, the thermometer

further knowledge of the nature of heat, to aſcertain, by indiſputable evidence, its paſſage through the Torricellian vacuum, and to determine, with as much preciſion as poſſible, the law of its motions in that medium; and being apprehenſive that doubts might ariſe with reſpect to the experiments before deſcribed, on account of the contact of the tubes of the incloſed thermometers in the inſtruments made uſe of with the containing glaſs globes, or rather with their cylinders; by which means it might be ſuſpected, that a certain quantity, if not all the heat acquired, might poſſibly be communicated: to put this matter beyond all doubt, I made the following experiment.

In the middle of a glaſs body, of a pear-like form, about 8 inches long, and 2½ inches in its greateſt diameter, I ſuſpended a ſmall mercurial thermometer, 5½ inches long, by a fine thread of ſilk, in ſuch a manner that neither the bulb of the thermometer, nor its tube, touched the containing glaſs body in any part. The tube of the thermometer was graduated, and marked with fine threads of ſilk of different colours bound round it, as in the thermometers belonging to the other inſtruments already deſcribed; and the thermometer was ſuſpended in its place by means of a ſmall ſteel ſpring, to which the end of the thread of ſilk which held the thermometer being attached, it (the ſpring) was forced into a ſmall globular protuberance or cavity, blown in the upper extremity of the glaſs body, about half an inch in diameter, where the ſpring remaining, the thermometer neceſſarily remained ſuſpended in the axis of the glaſs body. There was an opening at the bottom of the glaſs body, through which the thermometer was introduced; and a barometrical tube being ſoldered to this opening, the inſide of the glaſs body was voided of air by means of mercury; and this opening being afterwards ſealed hermetically, and the barometrical tube being taken away, the thermometer was left ſuſpended in a Torricellian vacuum.

In this inſtrument, as the incloſed thermometer did not touch the containing glaſs body in any part, on the contrary, being diſtant from its internal ſurface an inch or more in every part, it is clear, that whatever heat paſſed *into* or *out of* the thermometer muſt have paſſed *through* the ſurrounding Torricellian vacuum: for it cannot be ſuppoſed, that the fine thread of ſilk, by which the thermometer was ſuſpended, was capable of conducting any heat at all, or at leaſt any ſenſible quantity. I therefore flattered myſelf with hopes of being able, with the aſſiſtance thermometer N° 3. was conſtructed with a view to thoſe experiments; and having now provided myſelf with a ſtock of thoſe different kinds of airs, I began with *fixed air*, with which, by

aſſiſtance of this inſtrument, to determine poſitively with regard to the paſſage of heat in the Torricellian vacuum: and this, I think, I have done, notwithſtanding that an unfortunate accident put it out of my power to purſue the experiments ſo far as I intended.

This inſtrument being fitted to a ſmall ſtand or foot of wood, in ſuch a manner that the glaſs body remained in a perpendicular ſituation, I placed it in my room, by the ſide of another incloſed thermometer (N° 2.), which was ſurrounded by air, and obſerved the effect of the variation of heat in the atmoſphere. I ſoon diſcovered, by the motion of the mercury in the incloſed thermometer, that the heat paſſed through the Torricellian vacuum; but it appeared plainly from the ſluggiſhneſs or great inſenſibility of the thermometer, that the heat paſſed with much greater difficulty in this medium than in common air. I now plunged both the thermometers into a bucket of cold water; and I obſerved, that the mercury in the thermometer ſurrounded by air deſcended much faſter than that in the thermometer ſurrounded by the Torricellian vacuum. I took them out of the cold water, and plunged them into a veſſel of hot water (having no conveniencies at hand to repeat the experiment in form with the freezing and with the boiling water); and the thermometer ſurrounded by the Torricellian vacuum appeared ſtill to be much more inſenſible or ſluggiſh than that ſurrounded by air.

Theſe trials were quite ſufficient to convince me of the paſſage of heat in the Torricellian vacuum, and alſo of the greater difficulty of its paſſage in that medium than in common air; but, not ſatisfied to reſt my inquiries here, I took the firſt opportunity that offered, and ſet myſelf to repeat the experiments which I had before made with the inſtruments N° 1. and N° 2. I plunged this inſtrument into freezing water, where I let it remain till the mercury in the incloſed thermometer had deſcended to 0°; when, taking it out of the freezing water, I plunged it ſuddenly into a veſſel of boiling water, and prepared myſelf to obſerve the aſcent of the mercury in the incloſed thermometer as in the foregoing experiments; but unfortunately the moment the end of the glaſs body touched the boiling water, it cracked with the heat at the point where it had been hermetically ſealed, and the water ruſhing into the body, ſpoiled the experiment: and I have not ſince had an opportunity of providing myſelf with another inſtrument to repeat it.

means of water, I filled the globe and cylinder containing the thermometer; and ſtopping up the two holes in the great ſtopple cloſing the end of the cylinder, I expoſed the inſtrument in freezing water till the mercury in the incloſed thermometer had deſcended to 0°; when, taking it out of the freezing water, I plunged it into a large veſſel of boiling water, and prepared myſelf to obſerve the times of heating, as in the former caſes; but an accident happened, which ſuddenly put a ſtop to the experiment. Immediately upon plunging the inſtrument into the boiling water, the mercury began to riſe in the thermometer with ſuch uncommon celerity, that it had paſſed the firſt diviſion upon the tube (which marked the 10th degree, according to REAUMUR's ſcale) before I was aware of its being yet in motion; and having thus miſſed the opportunity of obſerving the time elapſed when the mercury arrived at that point, I was preparing to obſerve its paſſage of the next, when all of a ſudden the ſtopple cloſing the end of the cylinder was blown up the chimney with a great exploſion, and the thermometer, which, being cemented to it by its tube, was taken along with it, and was broken to pieces, and deſtroyed in its fall.

This unfortunate experiment, though it put a ſtop for the time to the inquiries propoſed, opened the way to other reſearches not leſs intereſting. Suſpecting that the exploſion was occaſioned by the rarefaction of the water which remained attached to the inſide of the globe and cylinder after the operation of filling them with fixed air; and thinking it more than probable, that the uncommon celerity, with which the mercury roſe in the thermometer, was principally owing to the ſame cauſe; I was led to examine the conducting power of *moiſt air*, or air ſaturated with water.

For this experiment I provided myſelf with a new thermometer N° 4. the bulb of which, being of the ſame form as thoſe already deſcribed (*viz.* globular) was alſo of the ſame ſize, or half an inch in diameter. To receive this thermometer a glaſs cylinder was provided, 8 lines in diameter, and about 14 inches long, and terminated at one end by a globe 1½ inch in diameter. In the center of this globe the bulb of the thermometer was confined, by means of the ſtopple which cloſed the end of the cylinder; which ſtopple, being near 2 inches long, received the end of the tube of the thermometer into a hole bored through its center or axis, and confined the thermometer in its place, without the aſſiſtance of any other apparatus. Through this ſtopple two other ſmall holes were bored, and lined with thin glaſs tubes, as in the thermometer N° 3. opening a paſſage into the cylinder, which holes were occaſionally ſtopped up with ſome ſtopples of cork; but to prevent accidents, ſuch as I had before experienced from an exploſion, great care was taken not to preſs theſe ſtopples into their places with any conſiderable force, that they might the more eaſily be blown out by any conſiderable effort of the confined air.

Though in this inſtrument the thermometer was not altogether ſo ſteady in its place as in the thermometers N° 1. N° 2. and N° 3. the elaſticity of the tube, and the weight of the mercury in the bulb of the thermometer, occaſioning a ſmall vibration or trembling of the thermometer upon any ſudden motion or jar; yet I preferred this method to the others, on account of the lower part of this thermometer being intirely free, or ſuſpended in ſuch a manner as not to touch, or have any communication with, the lower part of the cylinder or the globe: for though the quantity of heat received by the tube of the thermometer at its contact with the cylinder at its choaks, in the inſtruments N° 1. and N° 2. or with the branches of the ſteel ſpring in N° 3. and from thence communicated to the bulb, muſt have been exceedingly ſmall; yet I was deſirous to prevent even that, and every other poſſible error or inaccuracy, however ſmall, that might ariſe.

Does humidity augment the conducting power of air?

To determine this queſtion I made the following experiments, the weather being clear and fine, the mercury in the barometer ſtanding at 27 inches 8 lines, the thermometer at 19°, and the hygrometer at 44°.

(Exp. N° 17.)

Thermometer N° 4.

Surrounded by air *dry* to the 44th degree of the quill hygrometer of the Manheim Academy.

Taken out of freezing water, and plunged into boiling water.

(Exp. N° 18.)

The ſame thermometer (N° 4.)

Surrounded by air rendered as *moiſt* as poſſible by wetting the inſide of the cylinder and globe with water.

Taken out of freezing water, and plunged into boiling water.

Time elapſed.	Heat acquired.	Time elapſed.	Heat acquired.
—	80°	—	0°
M. S.	°	M. S.	°
0 34	10	0 6	10
0 39	20	0 4	20
0 44	30	0 5	30
0 51	40	0 9	40
1 6	50	0 18	50
1 35	60	0 26	60
2 40	70	0 43	70
not obſerved.	80	7 45	80

8 9 = total time of heating from 0° to 70°.

1 51 = total time of heating from 0° to 70°.

From theſe experiments, it appears, that the conducting power of air is very much increaſed by humidity. To ſee if the ſame reſult would obtain when the experiment was reverſed, I now took the thermometer with the *moiſt air* out of the boiling water, and plunged it into freezing water; and moving it about continually from place to place in the freezing water, I obſerved the times of cooling, as ſet down in the following table. N. B. To compare the reſult of this experiment with thoſe made with *dry air*, I have placed on one ſide in the following table the experiment in queſtion, and on the other ſide the experiment N° 19. made with the thermometer N° 2.

(Exp. N° 19.) *Thermometer* N° 4. Surrounded by *moiſt air*. *Taken out of boiling water, and plunged into freezing water.*			(Exp. N° 10.) *Thermometer* N° 2. Surrounded by *dry air*. *Taken out of boiling water, and plunged into freezing water.*		
Time elapſed.		Heat loſt.	Time elapſed.		Heat loſt.
		80°			80°
M.	S.	°	M.	S.	°
0	4	70	0	33	70
0	14	60	0	34	60
0	31	50	0	44	50
0	52	40	0	55	40
1	22	30	1	18	30
2	3	20	1	57	20
4	2	10	3	40	10
9	8	= total time of cooling from 80° to 10°.	9	12	= total time of cooling from 80° to 10°.

Though the difference of the whole times of cooling from 80° to 10° in theſe two experiments appears to have been very ſmall, yet the difference of the times taken up by the firſt twenty or thirty degrees from the boiling point is very remarkable, and ſhows with how much greater facility heat paſſes in moiſt air than in dry air. Even the ſlowneſs with which the mercury in the thermometer N° 4. deſcended in this experiment from the 30th to the 20th, and from the 20th to the 10th degree, I attribute in ſome meaſure to the great conducting power of the moiſt air with which it was ſurrounded; for the cylinder containing the thermometer and the moiſt air, being not wholly ſubmerged in the freezing water, that part of it which remained out of the water was neceſſarily ſurrounded by the air of the atmoſphere; which being much warmer than the water, communicated of its heat to the glaſs; which, paſſing from thence into the contained moiſt air as ſoon as that air became colder than the external air, was, through that medium, communicated to the bulb of the incloſed thermometer, which prevented its cooling ſo faſt as it would otherwiſe have done. But when the weather becomes cold, I propoſe to repeat this experiment with variations, in ſuch a manner as to put the matter beyond all doubt. In the mean time I cannot help obſerving, with what infinite wiſdom and goodneſs Divine Providence appears to have guarded us againſt the evil effects of exceſſive heat and cold in the atmoſphere; for if it were poſſible for the air to be equally damp during the ſevere cold of the winter months as it ſometimes is in ſummer, its conducting power, and conſequently its apparent coldneſs, when applied to our bodies, would be ſo much increaſed by ſuch an additional degree of moiſture, that it would become quite intolerable; but, happily for us, its power to hold water in ſolution is diminiſhed, and with it its power to rob us of our animal heat, in proportion as its coldneſs is increaſed. Every body knows how very diſagreeable a very moderate degree of cold is when the air is very damp; and from hence it appears, why the thermometer is not always a juſt meaſure of the apparent or ſenſible heat of the atmoſphere. If colds or catarrhs are occaſioned by our bodies being robbed of our animal heat, the reaſon is plain why thoſe diſorders prevail moſt during the cold autumnal rains, and upon the breaking up of the froſt in the ſpring. It is likewiſe plain from whence it is that ſleeping in damp beds, and inhabiting damp houſes, is ſo very dangerous; and why the evening air is ſo pernicious in ſummer and in autumn, and why it is not ſo during the hard froſts of winter. It has puzzled many very able philoſophers and phyſicians to account for the manner in which the extraordinary degree or rather *quantity* of heat is generated which an animal body is ſuppoſed to loſe, when expoſed to the cold of winter, above what it communicates to the ſurrounding atmoſphere in warm ſummer weather; but is it not more than probable, that the difference of the quantities of heat, actually loſt or communicated, is infinitely leſs than what they have imagined? Theſe inquiries are certainly very intereſting; and they are undoubtedly within the reach of well contrived and well conducted experiments. But taking my leave for the preſent of this curious ſubject of inveſtigation, I haſten to the ſequel of my experiments.

Finding ſo great a difference in the conducting powers of common air and of the Torricellian vacuum, I was led to examine the conducting powers of common air of different degrees of denſity. For this experiment I prepared the thermometer N°. 4. by ſtopping up one of the ſmall glaſs tubes paſſing through the ſtopple, and opening a paſſage into the cylinder, and by fitting a valve to the external overture of the other. The inſtrument, thus prepared, being put under the receiver of an air-pump, the air paſſed freely out of the globe and cylinder upon working the machine, but the valve above deſcribed prevented its return upon letting air into the receiver. The gage of the air-pump ſhowed the degree of rarity of the air under the receiver, and conſequently of that filling the globe and cylinder, and immediately ſurrounding the thermometer.

With this inſtrument, the weather being clear and fine, the mercury in the barometer ſtanding at 27 inches 9 lines, the thermometer at 15°, and the hygrometer at 47°, I made the following experiments.

(Exp. N° 20.) *Thermometer* N° 4. Surrounded by common air, barometer ſtanding at 27 inches 9 lines. *Taken out of freezing water, and plunged into boiling water.*		(Exp. N° 21.) *Thermometer* N° 4. Surrounded by air rarefied by pumping till the barometer-gage ſtood at 6 inches 11½ lines. *Taken out of freezing water, and plunged into boiling water.*		(Exp. N° 22.) *Thermometer* N° 4. Surrounded by air rarefied by pumping till the barometer-gage ſtood at 1 inch 2 lines. *Taken out of freezing water, and plunged into boiling water.*	
Time elapſed.	Heat acquired.	Time elapſed.	Heat acquired.	Time elapſed.	Heat acquired.
	0°		0°		0°
M. S.	°	M. S.	°	M. S.	°
0 31	10	0 31	10	0 29	10
0 40	20	0 38	20	0 36	20
0 41	30	0 44	30	0 49	30
0 47	40	0 51	40	1 1	40
1 4	50	1 7	50	1 1	50
1 25	60	1 19	60	1 24	60
2 28	70	2 27	70	2 31	70
10 17	80	10 21	80	not obſerved.	80
7 36 = total time of heating from 0° to 70°.		7 37 = total time of heating from 0° to 70°.		7 51 = total time of heating from 0° to 70°.	

The reſult of theſe experiments, I confeſs, ſurpriſed me not a little; but the diſcovery of truth being the ſole object of my inquiries (having no favourite theory to defend) it brings no diſappointment along with it, under whatever unexpected ſhape it may appear. I hope that further experiments may lead to the diſcovery of the cauſe why there is ſo little difference in the conducting powers of air of ſuch very different degrees of rarity, while there is ſo great a difference in the conducting powers of air, and of the Torricellian vacuum. At preſent, I ſhall not venture any conjectures upon the ſubject; but in the mean time I dare to aſſert, that the experiments I have made may be depended on.

The time of my ſtay at Manheim being expired (having had the honour to attend thither his moſt Serene Highneſs the Elector Palatine Duke of Bavaria, my moſt Gracious Maſter, in his late journey), I was prevented from purſuing theſe inquiries further at that time; but I ſhall not fail to recommence them the firſt leiſure time I can find, which I fancy will be about the beginning of the month of November. In the mean time, to enable myſelf to purſue them with effect, I am ſparing neither labour nor expence to provide a complete apparatus neceſſary for my purpoſe; and his Electoral Highneſs has been graciouſly pleaſed to order M. ARTARIA (who is in his ſervice) to come to Munich to aſſiſt me. With ſuch a Patron as his moſt Serene Highneſs, and with ſuch an aſſiſtant as ARTARIA, I ſhall go on in my purſuits with chearfulneſs. Would to God that my labours might be as uſeful to others as they will be pleaſant to me!

I ſhall conclude this letter with a ſhort account of ſome experiments I have made to determine the conducting powers of water and of mercury; and with a table, ſhowing at one view the conducting powers of all the different mediums which I have examined.

Having filled the glaſs globe incloſing the bulb of the thermometer N°. 4. firſt with water, and then with mercury, I made the following experiments, to aſcertain the conducting powers of thoſe two fluids.

(Exp. N° 23.) *Thermometer* N° 4. Surrounded by *water.* *Taken out of freezing water, and plunged into boiling water:*	
Time elapſed.	Heat acquired.
M. S.	0°
	0
0 19	10
0 8	20
0 9	30
0 11	40
0 15	50
0 21	60
0 34	70
2 13	80
1 57 = total time of heating from 0° to 70°.	

(Exp. N° 24, 25, and 26.) *Thermometer* N° 4. Surrounded by *mercury.* *Taken out of freezing water, and plunged into boiling water.*			
Time elapſed.			Heat acquired.
Exp. N° 24.	Exp. N° 25.	Exp. N° 26.	
			0°
M. S.	M. S.	M. S.	
0 5	0 5	0 5	10
0 4	0 2	0 5	20
0 2	0 2	0 4	30
0 4	0 5	0 5	40
0 4	0 4	0 7	50
0 7	0 4	0 8	60
0 15	0 9	0 14	70
Not obſerved.	0 58	Not obſerved.	80
0 41	0 31	0 48 = total times of heating from 0° to 70°.	

The total times of heating from 0° to 70° in the three experiments with mercury being 41 ſeconds, 31 ſeconds, and 48 ſeconds, the mean of theſe times is 36⅔ ſeconds; and as in the experiment with water the time employed in acquiring the ſame degree of heat was 1′ 57″ = 117 ſeconds, it appears from theſe experiments, that the conducting power of mercury to that of water, under the circumſtances deſcribed, is as $36\frac{2}{3}$ to 117 inverſely, or as 1000 to 313. And hence it is plain, why mercury *appears* ſo much hotter, and ſo much colder, to the touch than water, when in fact it is of the ſame temperature: for the force or violence of the ſenſation of *hot* or *cold* depends not intirely upon the temperature of the body exciting in us thoſe ſenſations, or upon the degree of heat it actually poſſeſſes, but upon the *quantity* of heat it is capable of communicating to us, or receiving from us, in any given ſhort period of time, or as the intenſity of the communication; and this depends in a great meaſure upon the conducting powers of the bodies in queſtion.

The ſenſation of *hot* is the entrance of heat into our bodies; that of *cold* is its exit; and whatever contributes to facilitate or accelerate this communication adds to the violence of the ſenſation. And this is another proof that the thermometer cannot be a juſt meaſure of the *ſenſible* heat, or cold, exiſting in bodies; or rather, that the touch does not afford us a juſt indication of their *real* temperatures.

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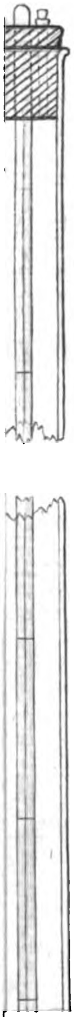
Philos. Trans. Vol. LXXVI. Tab. VI. p. 302.



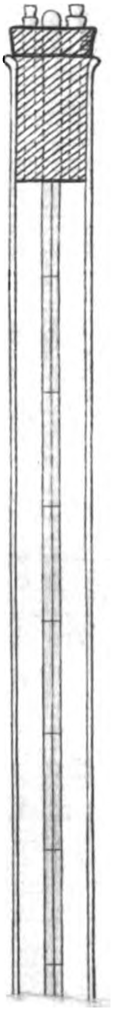


Fig. 4.

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A Table of the *conducting Powers* of the under-mentioned *Mediums*, as determined by the foregoing experiments.

Thermom. N° 1.	Thermometer N° 4.						
Taken out of freezing water, and plunged into boiling water.							
Time elapſed.							
Torricellian Vacuum (Exp. N° 3. 4. and 13.)	Common air, denſity = 1, (Exp. N° 20.)	Rarefied air, denſity = $\frac{1}{4}$ (Exp. N°. 21.)	Rarefied air, denſity = $\frac{1}{24}$ (Exp. N° 22.)	Moiſt air (Exp. N° 18.)	Water (Exp. N° 23.)	Mercury (Exp. N° 24, 25, and 26.)	Heat acquired.
—	—	—	—	—	—	—	0°
M. S.	M. S.	M. S.	M. S.	M. S.	M. S.	M. S.	
0 52	0 31	0 31	0 29	0 6	0 19	0 5	10°
0 58	0 40	0 38	0 36	0 4	0 8	0 3⅔	20
1 3	0 41	0 44	0 49	0 5	0 9	0 2⅔	30
1 18	0 47	0 51	1 1	0 9	0 11	0 4⅔	40
1 25	1 4	1 7	1 1	0 18	0 15	0 5	50
1 58	1 25	1 19	1 24	0 26	0 21	0 6⅓	60
3 19	2 28	2 27	2 31	0 43	0 34	0 12⅔	70
11 57	10 17	10 21	—	7 45	2 13	0 58	80
10 53	7 36	7 37	7 51	1 51	1 57	0 36⅔	= total times of heating from 0° to 70°.

In determining the relative conducting powers of theſe mediums, I have compared the times of the heating of the thermometers from 0° to 70° inſtead of taking the whole times from 0° to 80°, on account of the ſmall variation in the heat of the boiling water ariſing from the variation of the weight of the atmoſphere, and alſo on account of the very ſlow motion of the mercury between the 70th and the 80th degrees, and the difficulty of determining the preciſe moment when the mercury arrives at the 80th degree.

Taking now the conducting power of mercury = 1000, the conducting powers of the other mediums, as determined by theſe experiments, will be as follows, *viz.*

Mercury	1000
Moiſt air	330
Water,	313
Common air, denſity = 1	$80\frac{41}{100}$
Rarefied air, denſity = $\frac{1}{4}$	$80\frac{31}{100}$
Rarefied air, denſity = $\frac{1}{24}$	78
The Torricellian vacuum	55

And in theſe proportions are the quantities of heat which theſe different mediums are capable of tranſmitting in any given time; and conſequently theſe numbers expreſs the relative *ſenſible* temperatures of the mediums, as well as their conducting powers. How far theſe deciſions will hold good under a variation of circumſtances experiment only can determine. This is certainly a ſubject of inveſtigation not leſs curious in itſelf than it is intereſting to mankind; and I wiſh that what I have done may induce others to turn their attention to this long neglected field of experimental inquiry. For my own part, I am determined not to quit it.

In the future proſecution of theſe inquiries, I do not mean to confine myſelf ſolely to the determining of the conducting powers of fluids; on the contrary, ſolids, and particularly ſuch bodies as are made uſe of for cloathing, will be principal ſubjects of my future experiments. I have indeed already begun theſe reſearches, and have made ſome progreſs in them; but I forbear to anticipate a matter which I propoſe for the ſubject of a future communication.

⁂

XV. *Hiſtory and Diſſection of an extraordinary Introſuſception.* *By* John Coakley Lettſom, *M. D. F. R. S. and A. S.*

Read March 16, 1786.

A. B. a child four years old, was firſt indiſpoſed about the middle of September, 1784. When I was conſulted, which was on the 7th of October, the ſymptoms reſembled thoſe of a cholera morbus. At this period, however, the diarrhœa had ceaſed; but the patient continued frequently to vomit, eſpecially after taking nouriſhment.

On the 20th a dyſentery ſucceeded, with mucous and bloody ſtools, which ceaſed after a few days continuance, when ſhe nearly recovered her former ſtate of health, without even reaching after her uſual food. She was now removed into the country; and I did not hear of her again till December, when ſhe was brought to town, on account of the return of the dyſentery, which was, at this period, accompanied with a troubleſome teneſmus, and a conſiderable degree of fever.

By anodyne medicines, and the uſe of opiate clyſters, theſe complaints were occaſionally moderated, and ſometimes they totally ceaſed for a few days, but ſeldom longer, and the intervals of relief gradually ſhortened; the attacks became alſo more violent, commencing with violent rigors, and fever ſucceeding; the pulſe grew weaker and weaker, and the patient became extremely extenuated in fleſh; and towards the concluſion of this month, after repeated vomitings of a dark-coloured fluid, like coffee grounds, it finiſhed its painful exiſtence.

Bleeding, before the debility was become alarming, afforded no material reſpite. Fomentations to the abdomen, and tepid bathing of the whole body, were equally ineffectual. Anodyne ſtarch clyſters afforded ſome truce, but it could not be durable; the nature of the miſchief was too momentous to afford any hope of permanent relief, as the diſſection after death will evince.

Examination of the Body after Death, by Mr. THOMAS WHATELY, *Surgeon.*

Upon expoſing the cavity of the abdomen, the ſigmoid flexure of the colon immediately preſented itſelf to view, enlarged to the ſize of that of an adult, as alſo a large diſtended inteſtine, appearing to be at firſt view a continuation of the tranſverſe arch of this gut; and at the place where this ſeeming arch joined the ſigmoid flexure, there appeared a firm or tight band, as if ſurrounding the inteſtine, and here it was ſtrongly bound down.

On a nicer inſpection this arch was found to be a portion of the ileum, which paſſing within the band was incloſed in the ſigmoid flexure of the colon.

All the parts between this portion of the ſmall inteſtines and the ſigmoid flexure, amongſt which were the caput coli, cæcum with its appendix, and the whole of the great arch of the colon, could no where be ſeen. The want of theſe parts, the enlarged ſize of the ſigmoid flexure, and the hard feel evidently ſhewing that it contained ſome ſubſtance, left no room to doubt, but that all the miſſing portion of the inteſtines was contained within the ſigmoid flexure. A finger introduced into the anus felt a round ſubſtance in the rectum, with an opening in the middle, not unlike the os tincæ. This ſubſtance did not adhere, the finger paſſing round it freely, between it and the internal coat of the rectum. The liver, the urinary bladder, and ſmall inteſtines, were the remaining parts which firſt appeared when the parietes of the abdomen were turned back.

Upon looking for the omentum, a portion of it only was found attached to the ſtomach, the remaining part evidently paſſed within the band into the ſigmoid flexure.

The ſtomach was tied much cloſer to the ſpine than natural, by the diſplacing of the omentum and great arch of the colon. The gall bladder was as large as that of an adult, and was full of thin bile; but without obſtruction to its paſſage into the duodenum.

The general external appearance of all the inteſtines was natural, except ſlight inflammation in ſome places.

The cavity of the abdomen alſo contained more than half an ounce of thin pus; and on the right ſide were two ligamentous peritoneal ſubſtances, very much on the ſtretch; one formed by an extenſion of that part of the peritoneum called ligamentum * coli dextrum; the other at the place where the colon is connected to the peritoneum over the right kidney.

As the further inveſtigation of this uncommon diſeaſe required particular attention, I cut out all the parts connected with it, bringing away the whole ſigmoid flexure of the colon,

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* I have obſerved, that in ſome children the caput coli is naturally connected much more looſely than in others. It is probable, that the preſent caſe was one of thoſe.

with the rectum, anus, uterus, and bladder; alſo a part of the arch of the ileum with the omentum, and a portion of the ſtomach and duodenum.

The Drawing * (Tab. VII.) was taken by Mr. Pole, Surgeon, of the natural ſize, and the ſmall inteſtines added from a ſketch I made before the parts were removed from the body.

I then made a longitudinal inciſion through the coats of the ſigmoid flexure of the colon, from the anus to the band at its upper part. Within the cavity, which was lined with mucus, appeared a large inteſtine, taking on the form of the ſigmoid flexure, which on examination proved to be the great arch of the colon and the cæcum inverted; ſo that the villous coat was external, and in contact with the villous coat of the ſigmoid flexure, through the whole extent of both; at the extremity of which inverted inteſtine were two apertures, viz. the large one felt by the finger *per anum*, and a ſmaller one.

It now plainly appeared, that the band was formed by the upper part of the ſigmoid flexure being drawn tight by the inverſion of the part of the colon immediately above it, the further progreſs of which was prevented by the peritoneal connections at that place not giving way; which cauſed it to appear as a band tying the inteſtine down.

This incloſed inteſtine was very much diſeaſed, the upper part next the band being highly inflamed, and as it approached the caput coli in the rectum gradually terminated in mortification, ſo that for three inches from its extremity it was perfectly black.

No adheſion whatever appeared between the coats of theſe inteſtines, as this inverted colon might be lifted out of the ſigmoid flexure to the band.

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* Mr. Basire very accurately reduced the ſcale under my own inſpection from which the engravings are taken.

Upon cutting through the coats of this inverted inteſtine it was obſerved, that they were very much thickened and diſeaſed; the enlargement of the gut, which was fully equal to that of an adult, conſiſting chiefly in a thickening of its various muſcular fibres *. The peritoneal coat, now become its internal ſurface, was every where highly inflamed, but not black as on the outſide, the inflammation gradually increaſing from the band to the extremity of the cæcum. Through the whole length of its cavity was included a portion of the ileum uninverted, with its connecting meſentery, which communicated with the larger aperture above deſcribed at the extremity of the cæcum, and with the arch of the ileum above the band. It was contracted in ſize, but was nearly free from thickening or inflammation; ſome adheſions only connected it with the coats of the colon; but the portion above the band was at leaſt four times as large, thus reſembling in magnitude as well as occupying the place of the great arch of the colon. Beſides this inteſtine, this cavity contained a portion of the omentum continued from that above, paſſing within the band, and extending half-way to the rectum; an enlarged cluſter of meſenteric glands, of the ſize of a pigeon's egg, which juſt emerged from under the band, and were connected with a portion of the meſentery above; and, at the lower part, the appendix vermiformis larger and longer than natural, but likewiſe uninverted; the mouth of the cavity of which formed the ſmaller opening in the cæcum before mentioned. It was at this point of the diſſection that the ſame ingenious Surgeon drew the figure, tab. VIII.

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* The increaſed action of theſe muſcles, neceſſarily attendant on their inverted ſtate, would increaſe the ſize of their muſcular fibres, as happens in the bladder, when it acts frequently.

As long as the parts had been in this very uncommon ſituation, the fæces muſt have paſſed through the valve of the colon, directly into the very loweſt part of the rectum, without ever coming in contact with any portion of the large inteſtines.

And in the laſt week of the child's life, when a prolapſus frequently happened, the fæces paſſed directly from the ileum into the night-ſtool.

The arch of the ileum, in default of that of the colon, formed the reſervoir for the fæces; which, with the partial interruption to their paſſage by the ſtricture occaſioned by the band, probably cauſed its enlargement. But the fæces contained in it were of a thinner conſiſtence, and wanted the fætor uſually met with in the colon.

EXPLANATION OF THE PLATES.

TAB. VII.

A general view of the inteſtines, in the ſituation in which they appeared on firſt opening the body,

aa. The enlarged ileum, putting on the appearance of the great arch of the colon.

b. The ſudden enlargement of the ileum.

c. The ileum paſſing within the band into the colon.

d. Part of the omentum paſſing within the band.

e. The inteſtinal band, formed by the inverſion of the great arch of the colon immediately above it ceaſing at this place.

ff,

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ff. The ſigmoid flexure of the colon, containing the introſuſcepted portion of the alimentary canal.

g. The rectum diſtended with the ſame.

h. The anus.

ii. Small inteſtines of the natural ſize and healthy appearance.

TAB. VIII.

The ſame view, with the ſigmoid flexure laid open, and the edges turned back, to ſhew the contained parts; and likewiſe the introſuſcepted colon laid open, to diſplay the uninverted ileum and appendix vermiformis contained within it.

aaaa. Internal ſurface of the ſigmoid flexure of the colon ſpread open.

bbbb. The external ſurface (by the inverſion now become internal) of the great arch of the colon within the ſigmoid flexure ſpread open.

cc. Part of the ileum uninverted.

d. Appendix cæci uninverted.

ee. A probe piercing the diſtended ileum, paſſed within the band, and brought out in another portion of the ileum, contained within the inverted colon below the band.

ff. A blow-pipe paſſed through the valve of the colon, where it opened into the rectum, and brought out through the coats of the ileum above.

gg. A probe paſſed into the natural opening of the appendix cæci, and brought out above.

hh. The cæcum inverted.

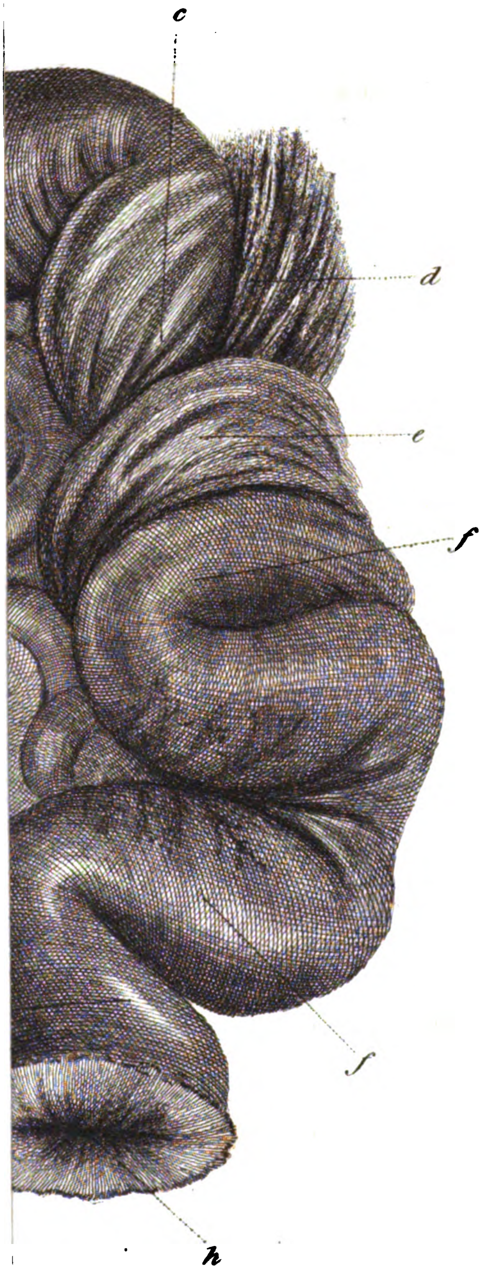
i.

i. Part of the rectum.

k. The anus.

l. Part of the omentum, attached to the peritoneal ſurface of the great arch of the colon, and continued from the portion above.

m. The cluſter of enlarged meſenteric glands.



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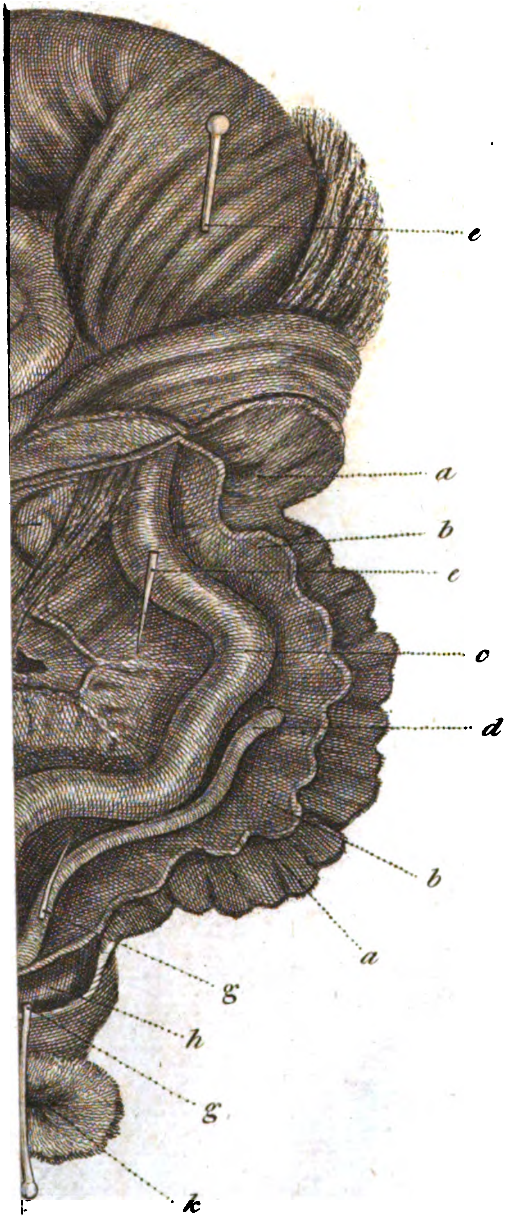

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XVI. *New Experiments on the Ocular Spectra of Light and Colours.* *By* Robert Waring Darwin, *M. D.*; *communicated by* Eraſmus Darwin, *M. D. F. R. S.*

Read March 23, 1786.

WHEN any one has long and attentively looked at a bright object, as at the ſetting ſun, on cloſing his eyes, or removing them, an image, which reſembles in form the object he was attending to, continues ſome time to be viſible: this appearance in the eye we ſhall call the ocular ſpectrum of that object.

Theſe ocular ſpectra are of four kinds: 1ſt, Such as are owing to a leſs ſenſibility of a defined part of the retina; or *ſpectra from defect of ſenſibility.* 2d, Such as are owing to a greater ſenſibility of a defined part of the retina; or *ſpectra from exceſs of ſenſibility.* 3d, Such as reſemble their object in its colour as well as form; which may be termed *direct ocular ſpectra.* 4th, Such as are of a colour contrary to that of their object; which may be termed *reverſe ocular ſpectra.*

The laws of light have been moſt ſucceſsfully explained by the great NEWTON, and the perception of viſible objects has been ably inveſtigated by the ingenious Dr. BERKELEY and M. MALEBRANCHE; but theſe minute phænomena of viſion have yet been thought reducible to no theory, though many philoſophers have employed a conſiderable degree of attention upon them: among theſe are Dr. JURIN, at the end of Dr. SMITH's Optics; M.

ÆPINUS, in the Nov. Com. Petropol. V. 10.; M. BEGUELIN, in the Berlin Memoires, V. II. 1771; M. D'ARCY, in the Hiſtoire de l'Acad. des Scienc. 1765; M. DE LA HIRE; and, laſtly, the celebrated M. DE BUFFON, in the Memoires de l'Acad. des Scien. who has termed them accidental colours, as if ſubjected to no eſtabliſhed laws, Ac. Par. 1743. M. p. 215.

I muſt here apprize the reader, that it is very difficult for different people to give the ſame names to various ſhades of colours; whence, in the following pages, ſomething muſt be allowed if, on repeating the experiments, the colours here mentioned ſhould not accurately correſpond with his own names of them.

I. ACTIVITY OF THE RETINA IN VISION.

From the ſubſequent experiments it appears, that the retina is in an active not in a paſſive ſtate during the exiſtence of theſe ocular ſpectra; and it is thence to be concluded, that all viſion is owing to the activity of this organ.

1. Place a piece of red ſilk, about an inch in diameter, as in fig. 1. (Tab. IX.) on a ſheet of white paper, in a ſtrong light; look ſteadily upon it from about the diſtance of half a yard for a minute; then cloſing your eyelids cover them with your hands, and a green ſpectrum will be ſeen in your eyes, reſembling in form the piece of red ſilk: after ſome time, this ſpectrum will diſappear and ſhortly re-appear; and this alternately three or four times, if the experiment is well made, till at length it vaniſhes entirely.

2. Place on a ſheet of white paper a circular piece of blue ſilk, about four inches in diameter, in the ſunſhine; cover the center of this with a circular piece of yellow ſilk, about

three

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three inches in diameter; and the center of the yellow ſilk with a circle of pink ſilk, about two inches in diameter; and the center of the pink ſilk with a circle of green ſilk, about one inch in diameter; and the center of this with a circle of indigo, about half an inch in diameter; make a ſmall ſpeck with ink in the very center of the whole, as in fig. 2.; look ſteadily for a minute on this central ſpot, and then cloſing your eyes, and applying your hand at about an inch diſtance before them, ſo as to prevent too much or too little light from paſſing through the eyelids, you will ſee the moſt beautiful circles of colours that imagination can conceive, which are moſt reſembled by the colours occaſioned by pouring a drop or two of oil on a ſtill lake in a bright day; but theſe circular iriſes of colours are not only different from the colours of the ſilks abovementioned, but are at the ſame time perpetually changing as long as they exiſt.

3. When any one in the dark preſſes either corner of his eye with his finger, and turns his eye away from his finger, he will ſee a circle of colours like thoſe in a peacock's tail: and a ſudden flaſh of light is excited in the eye by a ſtroke on it. (NEWTON's Opt. Qu. 16.)

4. When any one turns round rapidly on one foot, till he becomes dizzy and falls upon the ground, the ſpectra of the ambient objects continue to preſent themſelves in rotation, or appear to librate, and he ſeems to behold them for ſome time ſtill in motion.

From all theſe experiments it appears, that the ſpectra in the eye are not owing to the mechanical impulſe of light impreſſed on the retina, nor to its chemical combination with that organ, nor to the abſorption and emiſſion of light, as is obſerved in many bodies: for in all theſe caſes the ſpectra muſt

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either remain uniformly, or gradually diminiſh; and neither their alternate preſence and evaneſcence as in the firſt experiment, nor the perpetual changes of their colours as in the ſecond, nor the flaſh of light or colours in the preſſed eye as in the third, nor the rotation or libration of the ſpectra as in the fourth, could exiſt.

It is not abſurd to conceive, that the retina may be ſtimulated into motion, as well as the red and white muſcles which form our limbs and veſſels; ſince it conſiſts of fibres, like thoſe, intermixed with its medullary ſubſtance. To evince this ſtructure, the retina of an ox's eye was ſuſpended in a glaſs of warm water, and forcibly torn in a few places; the edges of theſe parts appeared jagged and hairy, and did not contract, and become ſmooth like ſimple mucus, when it is diſtended till it breaks; which ſhews that it conſiſts of fibres; and this its fibrous conſtruction became ſtill more diſtinct to the ſight, by adding ſome cauſtic alkali to the water, as the adhering mucus was firſt eroded, and the hair-like fibres remained floating in the veſſel. Nor does the degree of tranſparency of the retina invalidate the evidence of its fibrous ſtructure, ſince LEEUWENHOEK has ſhewn that the cryſtalline humour itſelf conſiſts of fibres. (Arcana Naturæ, V. 1. p. 70.)

Hence it appears, that as the muſcles have larger fibres intermixed with a ſmaller quantity of nervous medulla, the organ of viſion has a greater quantity of nervous medulla intermixed with ſmaller fibres; and it is probable, that the locomotive muſcles, as well as the vaſcular ones, of microſcopic animals have much greater tenuity than theſe of the retina.

And beſides the ſimilar laws, which will be ſhewn in this Paper to govern alike the actions of the retina and of the muſcles, there are many other analogies which exiſt between them. They are both originally excited into action by irritations, both act nearly in the ſame quantity of time, are alike ſtrengthened or fatigued by exertion, are alike painful if excited into action when they are in an inflamed ſtate, are alike liable to paralyſis, and to the torpor of old age.

II. OF SPECTRA FROM DEFECT OF SENSIBILITY.

The retina is not ſo eaſily excited into action by leſs irritation, after having been lately ſubjected to greater.

1. When any one paſſes from the bright daylight into a darkened room, the iriſes of his eyes expand themſelves to their utmoſt extent in a few ſeconds of time; but it is very long before the optic nerve, after having been ſtimulated by the greater light of the day, becomes ſenſible of the leſs degree of it in the room; and, if the room is not too obſcure, the iriſes will again contract themſelves in ſome degree, as the ſenſibility of the retina returns.

2. Place about half an inch ſquare of white paper on a black hat, and looking ſteadily on the center of it for a minute, remove your eyes to a ſheet of white paper; and after a ſecond or two a dark ſquare will be ſeen on the white paper, which will continue ſome time. A ſimilar dark ſquare will be ſeen in the cloſed eye, if light be admitted through the eye-lids.

So after looking at any luminous object of a ſmall ſize, as at the ſun, for a ſhort time, ſo as not much to fatigue the eyes, this part of the retina becomes leſs ſenſible to ſmaller quantities of light; hence, when the eyes are turned on other leſs luminous parts of the ſky, a dark ſpot is ſeen reſembling the ſhape of the ſun, or other luminous object which we laſt beheld. This is the ſource of one kind of the dark-coloured *muſcæ volitantes*. If this dark ſpot lies above the center of the eye, we turn our eyes that way, expecting to bring it into the center of the eye, that we may view it more diſtinctly; and in this caſe the dark ſpectrum ſeems to move upwards. If the dark ſpectrum is found beneath the center of the eye, we purſue it from the ſame motive, and it ſeems to move downwards. This has given riſe to various conjectures of ſomething floating in the aqueous humours of the eyes; but whoever, in attending to theſe ſpots, keeps his eyes unmoved by looking ſteadily at the corner of a cloud, at the ſame time that he obſerves the dark ſpectra, will be thoroughly convinced, that they have no motion but what is given to them by the movement of our eyes in purſuit of them. Sometimes the form of the ſpectrum, when it has been received from a circular luminous body, will become oblong; and ſometimes it will be divided into two circular ſpectra, which is owing to our changing the angle made by the two optic axiſes, according to the diſtance of the clouds or other bodies to which the ſpectrum is ſuppoſed to be contiguous. The apparent ſize of it will alſo be variable according to its ſuppoſed diſtance; but when ſuch a ſpectrum is received with only one eye, the other being covered, its form and number are invariable.

As theſe ſpectra are more eaſily obſervable when our eyes are a little weakened by fatigue, it has frequently happened, that people of delicate conſtitutions have been much alarmed at them, fearing a beginning decay of their ſight, and have thence fallen into the hands of ignorant oculiſts; but I believe they never are a prelude to any other diſeaſe of the eye, and that it is from habit alone, and our want of attention to them, that we do not ſee them on all objects every hour of our lives. But as the nerves of very weak people loſe their ſenſibility, in the ſame manner as their muſcles loſe their activity, by a ſmall time of exertion, it frequently happens, that ſick people in the extreme debility of fevers are perpetually employed in picking ſomething from the bed-cloaths, occaſioned by their miſtaking the appearance of theſe *muſcæ volitantes* in their eyes. BENVENUTO CELINI, an Italian artiſt, a man of ſtrong abilities, relates, that having paſſed the whole night on a diſtant mountain with ſome companions and a conjurer, and performed many ceremonies to raiſe the devil, on their return in the morning to Rome, and looking up when the ſun began to riſe, they ſaw numerous devils run on the tops of the houſes, as they paſſed along; ſo much were the ſpectra of their weakened eyes magnified by fear, and made ſubſervient to the purpoſes of fraud or ſuperſtition. (Life of BEN. CELINI.)

3. Place a ſquare inch of white paper on a large piece of ſtraw-coloured ſilk; look ſteadily ſome time on the white paper, and then move the center of your eyes on the ſilk, and a ſpectrum of the form of the paper will appear on the ſilk, of a deeper yellow than the other part of it: for the central part of the retina, having been ſome time expoſed to the ſtimulus of a greater quantity of white light, is become leſs ſenſible to a ſmaller quantity of it, and therefore ſees only the yellow rays in that part of the ſtraw-coloured ſilk.

Facts ſimilar to theſe are obſervable in other parts of our ſyſtem: thus, if one hand be made warm, and the other expoſed to the cold, and then both of them immerſed in ſubtepid water, the water is perceived warm to one hand, and cold to the other; and we are not able to hear weak ſounds for ſome time after we have been expoſed to loud ones; and we feel a chillineſs on coming into an atmoſphere of temperate warmth, after having been ſome time confined in a very warm room: and hence the ſtomach, and other organs of digeſtion, of thoſe who have been habituated to the greater ſtimulus of ſpirituous liquor, are not excited into their due action by the leſs ſtimulus of common food alone; of which the immediate conſequence is indigeſtion and hypochondriaciſm.

III. OF SPECTRA FROM EXCESS OF SENSIBILITY.

The retina is more eaſily excited into action by greater irritation after having been lately ſubjected to leſs.

1. If the eyes are cloſed, and covered perfectly with a hat, for a minute or two, in a bright day; on removing the hat a red or crimſon light is ſeen through the eye-lids. In this experiment the retina, after being ſome time kept in the dark, becomes ſo ſenſible to a ſmall quantity of light, as to perceive diſtinctly the greater quantity of red rays than of others which paſs through the eye-lids. A ſimilar coloured light is ſeen to paſs through the edges of the fingers, when the open hand is oppoſed to the flame of a candle.

2. If you look for ſome minutes ſteadily on a window in the beginning of the evening twilight, or in a dark day, and then move your eyes a little, ſo that thoſe parts of the retina, on which the dark frame-work of the window was delineated, may now fall on the glaſs part of it, many luminous lines, repreſenting the frame-work, will appear to lie acroſs the glaſs panes: for thoſe parts of the retina, which were before leaſt ſtimulated by the dark frame-work, are now more ſenſible to

light than the other parts of the retina which were expoſed to the more luminous parts of the window.

3. Make with ink on white paper a very black ſpot, about half an inch in diameter, with a tail about an inch in length, ſo as to repreſent a tadpole; look ſteadily for a minute on this ſpot, and, on moving the eye a little, the figure of the tadpole will be ſeen on the white part of the paper, which figure of the tadpole will appear whiter or more luminous than the other parts of the white paper; for the part of the retina on which the tadpole was delineated, is now more ſenſible to light than the other parts of it, which were expoſed to the white paper. This experiment is mentioned by Dr. IRWIN, but is not by him aſcribed to the true cauſe, namely, the greater ſenſibility of that part of the retina which has been expoſed to the black ſpot, than of the other parts which had received the white field of paper, which is put beyond a doubt by the next experiment.

4. On cloſing the eyes after viewing the black ſpot on the white paper, as in the foregoing experiment, a red ſpot is ſeen of the form of the black ſpot: for that part of the retina, on which the black ſpot was delineated, being now more ſenſible to light than the other parts of it, which were expoſed to the white paper, is capable of perceiving the red rays which penetrate the eyelids. If this experiment be made by the light of a tallow candle, the ſpot will be yellow inſtead of red; for tallow candles abound much with yellow light, which paſſes in greater quantity and force through the eyelids than blue light; hence the difficulty of diſtinguiſhing blue and green by this kind of candle light. The colour of the ſpectrum may poſſibly vary in the day light, according to the different colour of the meridian or the morning or evening light.

M. BEGUELIN, in the Berlin Memoires, V. II. 1771, obſerves, that, when he held a book ſo that the ſun ſhone upon his half-cloſed eyelids, the black letters, which he had long inſpected, became red, which muſt have been thus occaſioned. Thoſe parts of the retina which had received for ſome time the black letters, were ſo much more ſenſible than thoſe parts which had been oppoſed to the white paper, that to the former the red light, which paſſed through the eyelids, was perceptible. There is a ſimilar ſtory told, I think, in M. DE VOLTAIRE's Hiſtorical Works, of a Duke of Tuſcany, who was playing at dice with the general of a foreign army, and, believing he ſaw bloody ſpots upon the dice, portended dreadful events, and retired in confuſion. The obſerver, after looking for a minute on the black ſpots of a die, and careleſſly cloſing his eyes, on a bright day, would ſee the image of a die with red ſpots upon it, as above explained.

5. On emerging from a dark cavern, where we have long continued, the light of a bright day becomes intolerable to the eye for a conſiderable time, owing to the exceſs of ſenſibility exiſting in the eye, after having been long expoſed to little or no ſtimulus. This occaſions us immediately to contract the iris to its ſmalleſt aperture, which becomes again gradually dilated, as the retina becomes accuſtomed to the greater ſtimulus of the daylight.

The twinkling of a bright ſtar, or of a diſtant candle in the night, is perhaps owing to the ſame cauſe. While we continue to look upon theſe luminous objects, their central parts gradually appear paler, owing to the decreaſing ſenſibility of the part of the retina expoſed to their light; whilſt, at the ſame time, by the unſteadineſs of the eye, the edges of them are perpetually falling on parts of the retina that were juſt before expoſed to the darkneſs of the night, and therefore tenfold more ſenſible to light than the part on which the ſtar or candle had been for ſome time delineated. This pains the eye in a ſimilar manner as when we come ſuddenly from a dark room into bright daylight, and gives the appearance of bright ſcintillations. Hence the ſtars twinkle moſt when the night is darkeſt, and do not twinkle through teleſcopes, as obſerved by MUSSCHENBROECK; and it will afterwards be ſeen why this twinkling is ſometimes of different colours when the object is very bright, as Mr. MELVILL obſerved in looking at Sirius. For the opinions of others on this ſubject, ſee Dr. PRIESTLEY's valuable Hiſtory of Light and Colours, p. 494.

Many facts obſervable in the animal ſyſtem are ſimilar to theſe; as the hot glow occaſioned by the uſual warmth of the air, or our cloaths, on coming out of a cold bath; the pain of the fingers on approaching the fire after having handled ſnow; and the inflamed heels from walking in ſnow. Hence thoſe who have been expoſed to much cold have died on being brought to a fire, or their limbs have become ſo much inflamed as to mortify. Hence much food or wine given ſuddenly to thoſe who have almoſt periſhed by hunger has deſtroyed them; for all the organs of the famiſhed body are now become ſo much more irritable to the ſtimulus of food and wine, which they have long been deprived of, that inflammation is excited, which terminates in gangrene or fever.

IV. OF DIRECT OCULAR SPECTRA.

A quantity of ſtimulus ſomewhat greater than natural excites the retina into ſpaſmodic action, which ceaſes in a few ſeconds.

A certain duration and energy of the ſtimulus of light and colours excites the perfect action of the retina in viſion; for very quick motions are imperceptible to us, as well as very ſlow ones, as the whirling of a top, or the ſhadow on a ſun-dial. So perfect darkneſs does not affect the eye at all; and exceſs of light produces pain, not viſion.

1. When a fire-coal is whirled round in the dark, a lucid circle remains a conſiderable time in the eye; and that with ſo much vivacity of light, that it is miſtaken for a continuance of the irritation of the object. In the ſame manner, when a fiery meteor ſhoots acroſs the night, it appears to leave a long lucid train behind it, part of which, and perhaps ſometimes the whole, is owing to the continuance of the action of the retina after having been thus vividly excited. This is beautifully illuſtrated by the following experiment: fix a paper ſail, three or four inches in diameter, and made like that of a ſmoke jack, in a tube of paſteboard; on looking through the tube at a diſtant proſpect, ſome disjointed parts of it will be ſeen through the narrow intervals between the ſails; but as the fly begins to revolve, theſe intervals appear larger; and when it revolves quicker, the whole proſpect is ſeen quite as diſtinct as if nothing intervened, though leſs luminous.

2. Look through a dark tube, about half a yard long, at the area of a yellow circle of half an inch diameter, lying upon a blue area of double that diameter, for half a minute;

and on cloſing your eyes the colours of the ſpectrum will appear ſimilar to the two areas, as in fig. 3.; but if the eye is kept too long upon them, the colours of the ſpectrum will be the reverſe of thoſe upon the paper, that is, the internal circle will become blue, and the external area yellow; hence ſome attention is required in making this experiment.

3. Place the bright flame of a ſpermaceti candle before a black object in the night; look ſteadily at it for a ſhort time, till it is obſerved to become ſomewhat paler; and on cloſing the eyes, and covering them carefully, but not ſo as to compreſs them, the image of the blazing candle will continue diſtinctly to be viſible.

4. Look ſteadily, for a ſhort time, at a window in a dark day, as in Exp. 2. S. III. and then cloſing your eyes, and covering them with your hands, an exact delineation of the window remains for ſome time viſible in the eye. This experiment requires a little practice to make it ſucceed well; ſince, if the eyes are fatigued by looking too long on the window, or the day be too bright, the luminous parts of the window will appear dark in the ſpectrum, and the dark parts of the frame-work will appear luminous, as in Exp. 2. S. III. And it is even difficult for many, who firſt try this experiment, to perceive the ſpectrum at all; for any hurry of mind, or even too great attention to the ſpectrum itſelf, will diſappoint them, till they have had a little experience in attending to ſuch ſmall ſenſations.

The ſpectra deſcribed in this ſection, termed direct ocular ſpectra, are produced without much fatigue of the eye; the irritation of the luminous object being ſoon withdrawn, or its quantity of light being not ſo great as to produce any degree of uneaſineſs in the organ of viſion; which diſtinguiſhes them from the next claſs of ocular ſpectra, which are the conſequence of fatigue. Theſe direct ſpectra are beſt obſerved in ſuch circumſtances that no light, but what comes from the object, can fall upon the eye; as in looking through a tube, of half a yard long, and an inch wide, at a yellow paper on the ſide of a room, the direct ſpectrum was eaſily produced on cloſing the eye without taking it from the tube: but if the lateral light is admitted through the eye-lids, or by throwing the ſpectrum on white paper, it becomes a reverſe ſpectrum, as will be explained below.

The other ſenſes alſo retain for a time the impreſſions that have been made upon them, or the actions they have been excited into. So if a hard body is preſſed upon the palm of the hand, as is practiſed in tricks of legerdemain, it is not eaſy to diſtinguiſh for a few ſeconds whether it remains or is removed; and taſtes continue long to exiſt vividly in the mouth, as the ſmoke of tobacco, or the taſte of gentian, after the ſapid material is withdrawn.

V. *A quantity of ſtimulus ſomewhat greater than the laſt mentioned excites the retina into ſpaſmodic action, which ceaſes and recurs alternately.*

1. On looking for a time on the ſetting ſun, ſo as not greatly to fatigue the ſight, a yellow ſpectrum is ſeen when the eyes are cloſed and covered, which continues for a time, and then diſappears, and recurs repeatedly before it intirely vaniſhes. This yellow ſpectrum of the ſun when the eyelids are opened becomes blue; and if it is made to fall on the green graſs, or on other coloured objects, it varies its own

colour by an intermixture of theirs, as will be explained in another place.

2. Place a lighted ſpermaceti candle in the night about one foot from your eye, and look ſteadily on the center of the flame, till your eye becomes much more fatigued than in S. IV. Exp. 3.; and on cloſing your eyes a reddiſh ſpectrum will be perceived, which will ceaſe and return alternately.

The action of vomiting in like manner ceaſes, and is renewed by intervals, although the emetic drug is thrown up with the firſt effort: ſo after-pains continue ſome time after parturition; and the alternate pulſations of the heart of a viper are renewed for ſome time after it is cleared from its blood.

VI. OF REVERSE OCULAR SPECTRA.

The retina after having been excited into action by a ſtimulus ſomewhat greater than the laſt mentioned falls into oppoſite ſpaſmodic action.

The actions of every part of animal bodies may be advantageouſly compared with each other. This ſtrict analogy contributes much to the inveſtigation of truth; while thoſe looſer analogies, which compare the phænomena of animal life with thoſe of chemiſtry or mechanics, only ſerve to miſlead our inquiries.

When any of our larger muſcles have been in long or in violent action, and their antagoniſts have been at the ſame time extended, as ſoon as the action of the former ceaſes, the limb is ſtretched the contrary way for our eaſe, and a pandiculation or yawning takes place.

By the following obſervations it appears, that a ſimilar circumſtance obtains in the organ of viſion; after it has been fatigued by one kind of action, it ſpontaneouſly falls into the oppoſite kind.

1. Place a piece of coloured ſilk, about an inch in diameter, on a ſheet of white paper, about half a yard from your eyes; look ſteadily upon it for a minute; then remove your eyes upon another part of the white paper, and a ſpectrum will be ſeen of the form of the ſilk thus inſpected, but of a colour oppoſite to it. A ſpectrum nearly ſimilar will appear if the eyes are cloſed, and the eyelids ſhaded by approaching the hand near them, ſo as to permit ſome but to prevent too much light falling on them.

Red ſilk produced a green ſpectrum.
Green produced a red one.
Orange produced blue.
Blue produced orange.
Yellow produced violet.
Violet produced yellow.

That in theſe experiments the colours of the ſpectra are the reverſe of the colours which occaſioned them, may be ſeen by examining the third figure in Sir ISAAC NEWTON's Optics, L. II. p. 1. where thoſe thin laminæ of air, which reflected yellow, tranſmitted violet; thoſe which reflected red, tranſmitted a blue-green; and ſo of the reſt, agreeing with the experiments above related.

2. Theſe reverſe ſpectra are ſimilar to a colour, formed by a combination of all the primary colours except that with which the eye has been fatigued in making the experiment: thus the reverſe ſpectrum of red muſt be ſuch a green as would be produced by a combination of all the other priſmatic colours.

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To evince this fact the following ſatisfactory experiment was made. The priſmatic colours were laid on a circular paſteboard wheel, about four inches in diameter, in the proportions deſcribed in Dr. PRIESTLEY's Hiſtory of Light and Colours, pl. 12. fig. 83. except that the red compartment was intirely left out, and the others proportionably extended ſo as to complete the circle. Then, as the orange is a mixture of red and yellow, and as the violet is a mixture of red and indigo, it became neceſſary to put yellow on the wheel inſtead of orange, and indigo inſtead of violet, that the experiment might more exactly quadrate with the theory it was deſigned to eſtabliſh or confute; becauſe in gaining a green ſpectrum from a red object, the eye is ſuppoſed to have become inſenſible to red light. This wheel, by means of an axis, was made to whirl like a top; and on its being put in motion, a green colour was produced, correſponding with great exactneſs to the reverſe ſpectrum of red.

3. In contemplating any one of theſe reverſe ſpectra in the cloſed and covered eye, it diſappears and re-appears ſeveral times ſucceſſively, till at length it intirely vaniſhes, like the direct ſpectra in ſect. v.; but with this additional circumſtance, that when the ſpectrum becomes faint or evaneſcent, it is inſtantly revived by removing the hand from before the eyelids, ſo as to admit more light: becauſe then not only the fatigued part of the retina is inclined ſpontaneouſly to fall into motions of a contrary direction, but being ſtill ſenſible to all other rays of light, except that with which it was lately fatigued, is by theſe rays at the ſame time ſtimulated into thoſe motions which form the reverſe ſpectrum.

From theſe experiments there is reaſon to conclude, that the fatigued part of the retina throws itſelf into a contrary mode of action, like oſcitation or pandiculation, as ſoon as the ſtimulus which has fatigued it is withdrawn; and that it ſtill remains ſenſible, that is, liable to be excited into action by any other colours at the ſame time, except the colour with which it has been fatigued.

VII. *The retina after having been excited into action by a ſtimulus ſomewhat greater than the laſt mentioned falls into various ſucceſſive ſpaſmodic actions.*

1. On looking at the meridian ſun as long as the eyes can well bear its brightneſs, the diſc firſt becomes pale, with a luminous creſcent, which ſeems to librate from one edge of it to the other, owing to the unſteadineſs of the eye; then the whole phaſis of the ſun becomes blue, ſurrounded with a white halo; and on cloſing the eyes, and covering them with the hands, a yellow ſpectrum is ſeen, which in a little time changes into a blue one.

M. DE LA HIRE obſerved, after looking at the bright ſun, that the impreſſion in his eye firſt aſſumed a yellow appearance, and then green, and then blue; and wiſhes to aſcribe theſe appearances to ſome affection of the nerves. (PORTERFIELD on the Eye, Vol. I. p. 343.)

2. After looking ſteadily on about an inch ſquare of pink ſilk, placed on white paper, in a bright ſunſhine, at the diſtance of a foot from my eyes, and cloſing and covering my eyelids, the ſpectrum of the ſilk was at firſt a dark green, and the ſpectrum of the white paper became of a pink. The ſpectra then both diſappeared; and then the internal ſpectrum was blue; and then, after a ſecond diſappearance, became yellow,

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and laſtly pink, whilſt the ſpectrum of the field varied into red and green.

Theſe ſucceſſions of different coloured ſpectra were not exactly the ſame in the different experiments, though obſerved, as near as could be, with the ſame quantity of light, and other ſimilar circumſtances; owing, I ſuppoſe, to trying too many experiments at a time; ſo that the eye was not quite free from the ſpectra of the colours which were previouſly attended to.

The alternate exertions of the retina in the preceding ſection reſembled the oſcitation or pandiculation of the muſcles, as they were performed in directions contrary to each other, and were the conſequence of fatigue rather than of pain. And in this they differ from the ſucceſſive diſſimilar exertions of the retina, mentioned in this ſection, which reſemble in miniature the more violent agitations of the limbs in convulſive diſeaſes, as epilepſy, chorea S. Viti, and opiſthotonos; all which diſeaſes are perhaps, at firſt, the conſequence of pain, and have their periods afterwards eſtabliſhed by habit.

VIII. *The retina, after having been excited into action by a ſtimulus ſomewhat greater than the laſt mentioned, falls into a fixed ſpaſmodic action, which continues for ſome days.*

1. After having looked long at the meridian ſun, in making ſome of the preceding experiments, till the diſcs faded into a pale blue, I frequently obſerved a bright blue ſpectrum of the ſun on other objects all the next and the ſucceeding day, which conſtantly occurred when I attended to it, and frequently when I did not previouſly attend to it. When I cloſed and covered

my eyes, this appeared of a dull yellow; and at other times mixed with the colours of other objects on which it was thrown. It may be imagined, that this part of the retina was become inſenſible to white light, and thence a bluiſh ſpectrum became viſible on all luminous objects; but as a yellowiſh ſpectrum was alſo ſeen in the cloſed and covered eye, there can remain no doubt of this being the ſpectrum of the ſun. A ſimilar appearance was obſerved by M. ÆPINUS, which he acknowledges he could give no account of. (Nov. Com. Petrop. V. 10, p. 2. and 6.)

The locked jaw, and ſome cataleptic ſpaſms, are reſembled by this phænomenon; and from hence we may learn the danger to the eye by inſpecting very luminous objects too long a time.

IX. *A quantity of ſtimulus greater than the preceding induces a temporary paralyſis of the organ of viſion.*

1. Place a circular piece of bright red ſilk, about half an inch in diameter, on the middle of a ſheet of white paper; lay them on the floor in a bright ſunſhine, and fixing your eyes ſteadily on the center of the red circle, for three or four minutes, at the diſtance of four or ſix feet from the object, the red ſilk will gradually become paler, and finally ceaſe to appear red at all.

2. Similar to theſe are many other animal facts; as purges, opiates, and even poiſons, and contagious matter, ceaſe to ſtimulate our ſyſtem, after we have been habituated to their uſe. So ſome people ſleep undiſturbed by a clock, or even by a forge hammer in their neighbourhood: and not only continued irritations, but violent exertions of any kind, are ſucceeded

by temporary paralyſis. The arm drops down after violent action, and continues for a time uſeleſs; and it is probable, that thoſe who have periſhed ſuddenly in ſwimming, or in ſcating on the ice, have owed their deaths to the paralyſis, or extreme fatigue, which ſucceeds every violent and continued exertion.

X. MISCELLANEOUS REMARKS.

There were ſome circumſtances occurred in making theſe experiments, which were liable to alter the reſults of them, and which I ſhall here mention for the aſſiſtance of others, who may wiſh to repeat them.

1. *Of direct and inverſe ſpectra exiſting at the ſame time; of reciprocal direct ſpectra; of a combination of direct and inverſe ſpectra; of a ſpectral halo; rules to pre-determine the colours of ſpectra.*

a. When an area, about ſix inches ſquare, of bright pink Indian paper, had been viewed on an area, about a foot ſquare, of white writing paper, the internal ſpectrum in the cloſed eyes was green, being the reverſe ſpectrum of the pink paper; and the external ſpectrum was pink, being the direct ſpectrum of the pink paper. The ſame circumſtance happened when the internal area was white, and external one pink; that is, the internal ſpectrum was pink, and the external one green. All the ſame appearances occurred when the pink paper was laid on a black hat.

b. When ſix inches ſquare of deep violet poliſhed paper was viewed on a foot ſquare of white writing paper, the internal ſpectrum was yellow, being the reverſe ſpectrum of the violet paper, and the external one was violet, being the direct ſpectrum of the violet paper.

c. When ſix inches ſquare of pink paper was viewed on a foot ſquare of blue paper, the internal ſpectrum was blue, and the external ſpectrum was pink; that is, the internal one was the direct ſpectrum of the external object, and the external one was the direct ſpectrum of the internal object, inſtead of their being each the reverſe ſpectrum of the objects they belonged to.

d. When ſix inches ſquare of blue paper were viewed on a foot ſquare of yellow paper, the interior ſpectrum became a brilliant yellow, and the exterior one a brilliant blue. The vivacity of the ſpectra was owing to their being excited both by the ſtimulus of the interior and exterior objects; ſo that the interior yellow ſpectrum was both the reverſe ſpectrum of the blue paper, and the direct one of the yellow paper; and the exterior blue ſpectrum was both the reverſe ſpectrum of the yellow paper, and the direct one of the blue paper.

e. When the internal area was only a ſquare half-inch of red paper, laid on a ſquare foot of dark violet paper, the internal ſpectrum was green, with a reddiſh-blue halo. When the red internal paper was two inches ſquare, the internal ſpectrum was a deeper green, and the external one redder. When the internal paper was ſix inches ſquare, the ſpectrum of it became blue, and the ſpectrum of the external paper was red.

f. When a ſquare half-inch of blue paper was laid on a ſix-inch ſquare of yellow paper, the ſpectrum of the central paper in the cloſed eye was yellow, incircled with a blue halo. On looking long on the meridian ſun, the diſc fades into a pale blue ſurrounded with a whitiſh halo.

5 Theſe

Theſe circumſtances, though they very much perplexed the experiments till they were inveſtigated, admit of a ſatisfactory explanation; for while the rays from the bright internal object in exp. *a.* fall with their full force on the center of the retina, and, by fatiguing that part of it, induce the reverſe ſpectrum, many ſcattered rays, from the ſame internal pink paper, fall on the more external parts of the retina, but not in ſuch quantity as to occaſion much fatigue, and hence induce the direct ſpectrum of the pink colour in thoſe parts of the eye. The ſame reverſe and direct ſpectra occur from the violet paper in exp. *b.*: and in exp. *c.* the ſcattered rays from the central pink paper produce a direct ſpectrum of this colour on the external parts of the eye, while the ſcattered rays from the external blue paper produce a direct ſpectrum of that colour on the central part of the eye, inſtead of theſe parts of the retina falling reciprocally into their reverſe ſpectra. In exp. *d.* the colours being the reverſe of each other, the ſcattered rays from the exterior object falling on the central parts of the eye, and there exciting their direct ſpectrum, at the ſame time that the retina was excited into a reverſe ſpectrum by the central object, and this direct and reverſe ſpectrum being of ſimilar colour, the ſuperior brilliancy of this ſpectrum was produced. In exp. *e.* the effect of various quantities of ſtimulus on the retina, from the different reſpective ſizes of the internal and external areas, induced a ſpectrum of the internal area in the center of the eye, combined of the reverſe ſpectrum of that internal area and the direct one of the external area, in various ſhades of colour, from a pale green to a deep blue, with ſimilar changes in the ſpectrum of the external area. For the ſame reaſons, when an internal bright object was ſmall, as in exp. *f.*, inſtead of the whole of the ſpectrum of the external object, being,

being reverſe to the colour of the internal object, only a kind of halo, or radiation of colour, ſimilar to that of the internal object, was ſpread a little way on the external ſpectrum. For this internal blue area being ſo ſmall, the ſcattered rays from it extended but a little way on the image of the external area of yellow paper, and could therefore produce only a blue halo round the yellow ſpectrum in the center.

If any one ſhould ſuſpect that the ſcattered rays from the exterior coloured object do not intermix with the rays from the interior coloured object, and thus affect the central part of the eye, let him look through an opake tube, about two feet in length, and an inch in diameter, at a coloured wall of a room with one eye, and with the other eye naked; and he will find, that by ſhutting out the lateral light, the area of the wall ſeen through a tube appears as if illuminated by the ſunſhine, compared with the other parts of it; from whence ariſes the advantage of looking through a dark tube at diſtant paintings.

Hence we may ſafely deduce the following rules to determine before-hand the colours of all ſpectra. 1. *The direct ſpectrum* without any lateral light is an evaneſcent repreſentation of its object in the unfatigued eye. 2. With ſome lateral light it becomes of a colour combined of the direct ſpectrum of the central object, and of the circumjacent objects, in proportion to their reſpective quantity and brilliancy. 3. The *reverſe ſpectrum* without lateral light is a repreſentation in the fatigued eye of the form of its objects, with ſuch a colour as would be produced by all the primary colours, except that of the object. 4. With lateral light the colour is compounded of the reverſe ſpectrum of the central object, and the direct ſpectrum of the circumjacent objects, in proportion to their reſpective quantity and brilliancy.

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II. *Variation and vivacity of the ſpectra occaſioned by extraneous light.*

The reverſe ſpectrum, as has been before explained, is ſimilar to a colour, formed by a combination of all the primary colours, except that with which the eye has been fatigued in making the experiment: ſo the reverſe ſpectrum of red is ſuch a green as would be produced by a combination of all the other priſmatic colours. Now it muſt be obſerved, that this reverſe ſpectrum of red is therefore the direct ſpectrum of a combination of all the other priſmatic colours, except the red; whence, on removing the eye from a piece of red ſilk to a ſheet of white paper, the green ſpectrum, which is perceived, may either be called the reverſe ſpectrum of the red ſilk, or the direct ſpectrum of all the rays from the white paper, except the red; for in truth it is both. Hence we ſee the reaſon why it is not eaſy to gain a direct ſpectrum of any coloured object in the day-time, where there is much lateral light, except of very bright objects, as of the ſetting ſun, or by looking through an opake tube; becauſe the lateral external light falling alſo on the central part of the retina, contributes to induce the reverſe ſpectrum, which is at the ſame time the direct ſpectrum of that lateral light, deducting only the colour of the central object which we have been viewing. And for the ſame reaſon, it is difficult to gain the reverſe ſpectrum, where there is no lateral light to contribute to its formation. Thus, in looking through an opake tube on a yellow wall, and cloſing my eye, without admitting any lateral light, the ſpectra were all at firſt yellow; but at length changed into blue. And on looking in the ſame manner on red paper, I did at length get a green

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green ſpectrum; but they were all at firſt red ones: and the ſame after looking at a candle in the night.

The reverſe ſpectrum was formed with greater facility when the eye was thrown from the object on a ſheet of white paper, or when light was admitted through the cloſed eyelids; becauſe not only the fatigued part of the retina was inclined ſpontaneouſly to fall into motions of a contrary direction; but being ſtill ſenſible to all other rays of light except that with which it was lately fatigued, was by theſe rays ſtimulated at the ſame time into thoſe motions which form the reverſe ſpectrum. Hence, when the reverſe ſpectrum of any colour became faint, it was wonderfully revived by admitting more light through the eyelids, by removing the hand from before them: and hence, on covering the cloſed eyelids, the ſpectrum would often ceaſe for a time, till the retina became ſenſible to the ſtimulus of the ſmaller quantity of light, and then it recurred. Nor was the ſpectrum only changed in vivacity, or in degree, by this admiſſion of light through the eyelids; but it frequently happened, after having viewed bright objects, that the ſpectrum in the cloſed and covered eye was changed into a third ſpectrum, when light was admitted through the eyelids: which third ſpectrum was compoſed of ſuch colours as could paſs through the eyelids, except thoſe of the object. Thus, when an area of half an inch diameter of pink paper was viewed on a ſheet of white paper in the ſunſhine, the ſpectrum with cloſed and covered eyes was green; but on removing the hands from before the cloſed eyelids, the ſpectrum became yellow, and returned inſtantly again to green, as often as the hands were applied to cover the eyelids, or removed from them: for the retina being now inſenſible to red light, the yellow rays paſſing through the eyelids in greater quantity than the other colours, induced a yellow ſpectrum; whereas if the

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the ſpectrum was thrown on white paper, with the eyes open, it became only a lighter green.

Though a certain quantity of light facilitates the formation of the reverſe ſpectrum, a greater quantity prevents its formation, as the more powerful ſtimulus excites even the fatigued parts of the eye into action; otherwiſe we ſhould ſee the ſpectrum of the laſt viewed object as often as we turn our eyes. Hence the reverſe ſpectra are beſt ſeen by gradually approaching the hand near the cloſed eyelids to a certain diſtance only, which muſt be varied with the brightneſs of the day, or the energy of the ſpectrum. Add to this, that all dark ſpectra, as black, blue, or green, if light be admitted through the eyelids, after they have been ſome time covered, give reddiſh ſpectra, for the reaſons given in ſect. III. exp. 1.

From theſe circumſtances of the extraneous light coinciding with the ſpontaneous efforts of the fatigued retina to produce a reverſe ſpectrum, as was obſerved before, it is not eaſy to gain a direct ſpectrum, except of objects brighter than the ambient light; ſuch as a candle in the night, the ſetting ſun, or viewing a bright object through an opake tube; and then the reverſe ſpectrum is inſtantaneouſly produced by the admiſſion of ſome external light; and is as inſtantly converted again to the direct ſpectrum by the excluſion of it. Thus, on looking at the ſetting ſun, on cloſing the eyes, and covering them, a yellow ſpectrum is ſeen, which is the direct ſpectrum of the ſetting ſun; but on opening the eyes on the ſky, the yellow ſpectrum is immediately changed into a blue one, which is the reverſe ſpectrum of the yellow ſun, or the direct ſpectrum of the blue ſky, or a combination of both. And this is again transformed into a yellow one on cloſing the eyes, and ſo reciprocally, as quick as the motions of the opening and cloſing eyelids. Hence, when Mr. MELVILL obſerved the ſcintillations of the ſtar Sirius to be ſometimes coloured, theſe were probably the direct ſpectrum of the blue ſky on the parts of the retina fatigued by the white light of the ſtar. (Eſſays Phyſical and Literary, p. 81. V. 2.)

When a direct ſpectrum is thrown on colours darker than itſelf, it mixes with them; as the yellow ſpectrum of the ſetting ſun, thrown on the green graſs, becomes a greener yellow. But when a direct ſpectrum is thrown on colours brighter than itſelf, it becomes inſtantly changed into the reverſe ſpectrum, which mixes with thoſe brighter colours. So the yellow ſpectrum of the ſetting ſun thrown on the luminous ſky becomes blue, and changes with the colour or brightneſs of the clouds on which it appears. But the reverſe ſpectrum mixes with every kind of colour on which it is thrown, whether brighter than itſelf or not: thus the reverſe ſpectrum, obtained by viewing a piece of yellow ſilk, when thrown on white paper was a lucid blue green; when thrown on black Turkey leather becomes a deep violet. And the ſpectrum of blue ſilk, thrown on white paper, was a light yellow; on black ſilk was an obſcure orange; and the blue ſpectrum, obtained from orange-coloured ſilk, thrown on yellow, became a green.

In theſe caſes the retina is thrown into activity or ſenſation by the ſtimulus of external colours, at the ſame time that it continues the activity or ſenſation which forms the ſpectra; in the ſame manner as the priſmatic colours, painted on a whirling top, are ſeen to mix together. When theſe colours of external objects are brighter than the direct ſpectrum which is thrown upon them, they change it into the reverſe ſpectrum, like the admiſſion of external light on a direct ſpectrum, as explained above. When they are darker than the direct ſpectrum, they mix with it, their weaker ſtimulus being inſufficient to induce the reverſe ſpectrum.

III. *Variation of ſpectra in reſpect to number and figure and remiſſion.*

When we look long and attentively at any object, the eye cannot always be kept intirely motionleſs; hence, on inſpecting a circular area of red ſilk placed on white paper, a lucid creſcent or edge is ſeen to librate on one ſide or other of the red circle: for the exterior parts of the retina ſometimes falling on the edge of the central ſilk, and ſometimes on the white paper, are leſs fatigued with red light than the central part of the retina, which is conſtantly expoſed to it; and therefore, when they fall on the edge of the red ſilk, they perceive it more vividly. Afterwards, when the eye becomes fatigued, a green ſpectrum in the form of a creſcent is ſeen to librate on one ſide or other of the central circle, as by the unſteadineſs of the eye a part of the fatigued retina falls on the white paper; and as by the increaſing fatigue of the eye the central part of the ſilk appears paler, the edge on which the unfatigued part of the retina occaſionally falls will appear of a deeper red than the original ſilk, becauſe it is compared with the pale internal part of it. M. DE BUFFON in making this experiment obſerved, that the red edge of the ſilk was not only deeper coloured than the original ſilk; but, on his retreating a little from it, it became oblong, and at length divided into two, which muſt have been owing to a change of the angle of the two optic axiſes with the new diſtance he obſerved it at. Thus, if a pen is held up before a diſtant candle, when we look intenſely at the pen two candles are ſeen behind it; when we look intenſely at the candle two pens are ſeen. If the ſight be unſteady at the time of beholding the ſun, even though one eye only be uſed, many images of the ſun will appear, or luminous lines, when the eye is cloſed. And as ſome parts of theſe will be more vivid than others, and ſome parts of them will be produced nearer the center of the eye than others, theſe will diſappear ſooner than the others; and hence the number and ſhape of theſe ſpectra of the ſun will continually vary, as long as they exiſt. The cauſe of ſome being more vivid than others, is the unſteadineſs of the eye of the beholder, ſo that ſome parts of the retina have been longer expoſed to the ſunbeams. That ſome parts of a complicated ſpectrum fade and return before other parts of it, the following experiment evinces. Draw three concentric circles; the external one an inch and a half in diameter, the middle one an inch, and the internal one half an inch; colour the external and internal areas blue, and the remaining one yellow, as in fig. 4.; after having looked about a minute on the center of theſe circles, in a bright light, the ſpectrum of the external area appears firſt in the cloſed eye, then the middle area, and laſtly the central one; and then the central one diſappears, and the others in inverted order. If concentric circles of more colours are added, it produces the beautiful ever changing ſpectrum in ſect. I. exp. 2.

From hence it would ſeem, that the center of the eye produces quicker remiſſions of ſpectra, owing perhaps to its greater ſenſibility; that is, to its more energetic exertions. Theſe remiſſions of ſpectra bear ſome analogy to the tremors of the hands, and palpitations of the heart, of weak people: and perhaps a criterion of the ſtrength of any muſcle or nerve may be taken from the time it can be continued in exertion.

IV. *Variation of ſpectra in reſpect to brilliancy; the viſibility of the circulation of the blood in the eye.*

1. The meridian or evening light makes a difference in the colours of ſome ſpectra; for as the ſun deſcends, the red rays, which are leſs refrangible by the convex atmoſphere, abound in great quantity. Whence the ſpectrum of the light parts of a window at this time, or early in the morning, is red; and becomes blue either a little later or earlier; and white in the meridian day; and is alſo variable from the colour of the clouds or ſky which are oppoſed to the window.

2. All theſe experiments are liable to be confounded, if they are made too ſoon after each other, as the remaining ſpectrum will mix with the new ones. This is a very troubleſome circumſtance to painters, who are obliged to look long upon the ſame colour; and in particular to thoſe whoſe eyes, from natural debility, cannot long continue the ſame kind of exertion. For the ſame reaſon, in making theſe experiments, the reſult becomes much varied if the eyes, after viewing any object, are removed on other objects for but an inſtant of time, before we cloſe them to view the ſpectrum; for the light from the object, of which we had only a tranſient view, in the very time of cloſing our eyes acts as a ſtimulus on the fatigued retina; and for a time prevents the deſired ſpectrum from appearing, or mixes its own ſpectrum with it. Whence, after the eyelids are cloſed, either a dark field, or ſome unexpected colours, are beheld for a few ſeconds, before the deſired ſpectrum becomes diſtinctly viſible.

3. The length of time taken up in viewing an object, of which we are to obſerve the ſpectrum, makes a great difference

in the appearance of the ſpectrum, not only in its vivacity, but in its colour; as the direct ſpectrum of the central object, or of the circumjacent ones, and alſo the reverſe ſpectra of both, with their various combinations, as well as the time of their duration in the eye, and of their remiſſions or alternations, depend upon the degree of fatigue the retina is ſubjected to. The Chevalier D'ARCY conſtructed a machine by which a coal of fire was whirled round in the dark, and found, that when a luminous body made a revolution in eight thirds of time, it preſented to the eye a complete circle of fire; from whence he concludes, that the impreſſion continues on the organ about the ſeventh part of a ſecond. (Mém. de l'Acad. des Sc. 1765.) This, however, is only to be conſidered as the ſhorteſt time of the duration of theſe direct ſpectra; ſince in the fatigued eye both the direct and reverſe ſpectra, with their intermiſſions, appear to take up many ſeconds of time, and ſeem very variable in proportion to the circumſtances of fatigue or energy.

4. It ſometimes happens, if the eyeballs have been rubbed hard with the fingers, that lucid ſparks are ſeen in quick motion amidſt the ſpectrum we are attending to. This is ſimilar to the flaſhes of fire from a ſtroke on the eye in fighting, and is reſembled by the warmth and glow which appear upon the ſkin after friction, and is probably owing to an acceleration of the arterial blood into the veſſels emptied by the previous preſſure. By being accuſtomed to obſerve ſuch ſmall ſenſations in the eye, it is eaſy to ſee the circulation of the blood in this organ. I have attended to this frequently, when I have obſerved my eyes more than commonly ſenſible to other ſpectra. The circulation may be ſeen either in both eyes at a time, or only in one of them; for as a certain quantity of light is neceſſary

to

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to produce this curious phænomenon, if one hand be brought nearer the cloſed eyelids than the other, the circulation in that eye will for a time diſappear. For the eaſier viewing the circulation, it is ſometimes neceſſary to rub the eyes with a certain degree of force after they are cloſed, and to hold the breath rather longer than is agreeable, which, by accumulating more blood in the eye, facilitates the experiment; but in general it may be ſeen diſtinctly after having examined other ſpectra with your back to the light, till the eyes become weary; then having covered your cloſed eyelids for half a minute, till the ſpectrum is faded away which you were examining, turn your face to the light, and removing your hands from the eyelids, by and by again ſhade them a little, and the circulation becomes curiouſly diſtinct. The ſtreams of blood are however generally ſeen to unite, which ſhews it to be the venous circulation, owing, I ſuppoſe, to the greater opacity of the colour of the blood in theſe veſſels; for this venous circulation is alſo much more eaſily ſeen by the microſcope in the tail of a tadpole.

V. *Variation of ſpectra in reſpect to diſtinctneſs and ſize; with a new way of magnifying objects.*

1. It was before obſerved, that when the two colours viewed together were oppoſite to each other, as yellow and blue, red and green, &c. according to the table of reflections and tranſmiſſions of light in Sir Isaac Newton's Optics, B. II. fig. 3. the ſpectra of thoſe colours were of all others the moſt brilliant, and beſt defined; becauſe they were combined of the reverſe ſpectrum of one colour, and of the direct ſpectrum of the other. Hence, in books printed with ſmall types, or in

the minute graduation of thermometers, or of clock-faces, which are to be ſeen at a diſtance, if the letters or figures are coloured with orange, and the ground with indigo; or the letters with red, and the ground with green; or any other lucid colour is uſed for the letters, the ſpectrum of which is ſimilar to the colour of the ground; ſuch letters will be ſeen much more diſtinctly, and with leſs confuſion, than in black or white: for as the ſpectrum of the letter is the ſame colour with the ground on which they are ſeen, the unſteadineſs of the eye in long attending to them will not produce coloured lines by the edges of the letters, which is the principal cauſe of their confuſion. The beauty of colours lying in vicinity to each other, whoſe ſpectra are thus reciprocally ſimilar to each colour, is owing to this greater eaſe that the eye experiences in beholding them diſtinctly; and it is probable, in the organ of hearing a ſimilar circumſtance may conſtitute the pleaſure of melody. Sir ISAAC NEWTON obſerves, that gold and indigo were agreeable when viewed together; and thinks there may be ſome analogy between the ſenſations of light and ſound. (Optics, Qu. 14.)

In viewing the ſpectra of bright objects, as of an area of red ſilk of half an inch diameter on white paper, it is eaſy to magnify it to tenfold its ſize: for if, when the ſpectrum is formed, you ſtill keep your eye fixed on the ſilk area, and remove it a few inches further from you, a green circle is ſeen round the red ſilk: for the angle now ſubtended by the ſilk is leſs than it was when the ſpectrum was formed, but that of the ſpectrum continues the ſame, and our imagination places them at the ſame diſtance. Thus when you view a ſpectrum on a ſheet of white paper, if you approach the paper to the eye, you may diminiſh it to a point; and if the paper is made to recede from the eye, the ſpectrum will appear magnified in proportion to the diſtance.

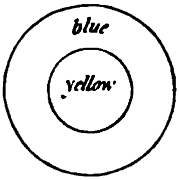
I was ſurpriſed, and agreeably amuſed, with the following experiment. I covered a paper about four inches ſquare with yellow, and with a pen filled with a blue colour wrote upon the middle of it the word BANKS in capitals, as in fig. 5. and, ſitting with my back to the ſun, fixed my eyes for a minute exactly on the center of the letter N in the middle of the word; after cloſing my eyes, and ſhading them ſomewhat with my hand, the word was diſtinctly ſeen in the ſpectrum in yellow letters on a blue field; and then, on opening my eyes on a yellowiſh wall at twenty feet diſtance, the magnified name of BANKS appeared written on the wall in golden characters.

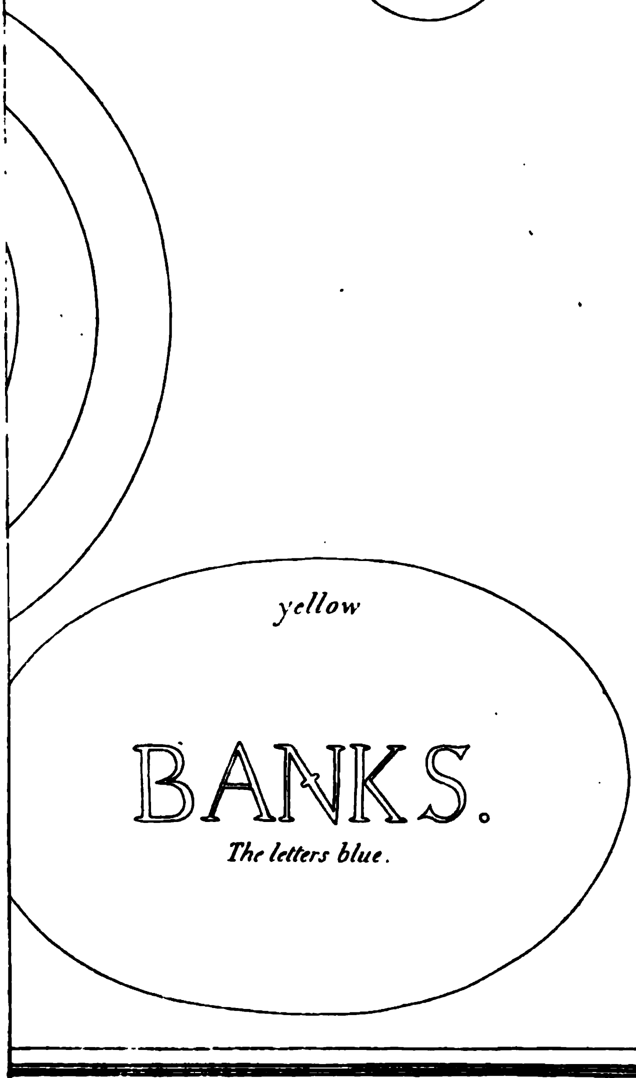
CONCLUSION.

It was obſerved by the learned M. SAUVAGES (Noſol. method. Cl. VIII. Ord. 1.) that the pulſations of the optic artery might be perceived by looking attentively on a white wall well illuminated. A kind of net-work, darker than the other parts of the wall, appears and vaniſhes alternately with every pulſation. This change of the colour of the wall he well aſcribes to the compreſſion of the retina by the diaſtole of the artery. The various colours produced in the eye by the preſſure of the finger, or by a ſtroke on it, as mentioned by Sir ISAAC NEWTON, ſeem likewiſe to originate from the unequal preſſure on various parts of the retina. Now as Sir ISAAC NEWTON has ſhewn, that all the different colours are reflected or tranſmitted by the laminæ of ſoap bubbles, or of air, according to their different thickneſs or thinneſs, is it not probable, that the effect of the activity of the retina may be to alter its thickneſs or thinneſs, ſo as better to adapt it to reflect or tranſmit the colours which ſtimulate it into action? May not muſcular fibres exiſt in the retina for this purpoſe, which may be leſs minute than the locomotive muſcles of microſcopic animals? May not theſe muſcular actions of the retina conſtitute the ſenſation of lights and colours; and the voluntary repetitions of them, when the object is withdrawn, conſtitute our memory of them? And laſtly, may not the laws of the ſenſations of light, here inveſtigated, be applicable to all our other ſenſes, and much contribute to elucidate many phænomena of animal bodies both in their healthy and diſeaſed ſtate; and thus render this inveſtigation well worthy the attention of the phyſician, the metaphyſician, and the natural philoſopher?

Derby, November 1, 1785.

Fig. 3.





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XVII. *Obſervations on ſome Cauſes of the Exceſs of the Mortality of Males above that of Females. By* Joſeph Clarke, *M. D. Phyſician to the Lying-in Hoſpital at* Dublin. *Communicated by the Rev.* Richard Price, *D. D. F. R. S. in a Letter to* Charles Blagden, *M. D. Sec. R. S.*

Read March 30, 1786.

SIR, Newington-Green, February 6, 1786.

I RECEIVED ſome time ago the incloſed letters and regiſtry from Dr. CLARKE, Phyſician to the Lying-in Hoſpital at Dublin. They contain ſome accounts that ſeem to me not improper to be communicated to the Royal Society.

The obſervations which have been made on the laws that govern human mortality prove, that the mortality of males exceeds that of females in almoſt all the ſtages of life, and particularly in the earlieſt ſtages; and that this exceſs prevails moſt in great towns, and all the leſs natural ſituations of human life. The facts in theſe papers throw ſome light on this ſubject. Male *fœtus's* requiring more nutrition than female *fœtus's*, becauſe larger, and being alſo for this reaſon more liable to injury in delivery, are brought into the world leſs perfect: and this happening more or leſs in proportion to the vigour and juſt formation of the mother, it muſt happen moſt in thoſe ſituations where the greateſt tenderneſs of frame and deviations from nature take place. The truth, in ſhort, ſeems to be, that any debility in *either* parent muſt affect moſt the production of that ſex which requires the largeſt and ſtrongeſt *ſtamina*; and that ſuch debilities prevailing moſt in great towns and poliſhed Societies, the exceſs of the mortality of males muſt alſo be greateſt in ſuch ſituations. And this I reckon the principal reaſon of a circumſtance in human mortality which, before I received theſe communications from Dr. CLARKE, I did not ſo well underſtand.

With much reſpect I am, &c.

RICH. PRICE.

Dr. CLARKE'S *firſt Letter to the Rev. Dr.* PRICE.

SIR, Dublin.

IN your very uſeful Treatiſe on Life Annuities, &c. you remark *, that "it has been obſerved, that the Author of "nature has provided, that more *males* ſhould be born than "females, on account of the particular waſte of males, occa-"ſioned by wars and other cauſes. That perhaps it might "have been obſerved, with more reaſon, that this proviſion "had in view that particular weakneſs or delicacy in the con-"ſtitution of males which makes them more ſubject to mor-"tality; and which, conſequently, renders it neceſſary that "more of them ſhould be produced, in order to preſerve

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* Vol. I. p. 373.

"in the world a due proportion between the ſexes." And further, you elſewhere remark *, that "the *facts* recited at "the end of your fourth Eſſay *prove*, that there is a difference "between the mortality of males and females; but that you "muſt however obſerve, that it may be *doubted*, whether this "difference, ſo unfavourable to males, be *natural*; and that "there are facts which prove that you have reaſon for ſuch a "doubt." After ſtating a number of very ſatisfactory facts of this kind you remark, that "the inference from them is very "obvious; that they ſeem to ſhew ſufficiently, that human "life in males is more brittle than in females, only in conſequence of adventitious cauſes, or of ſome particular debility "which takes place in poliſhed and luxurious ſocieties, and "eſpecially in great *towns*."

What thoſe adventitious cauſes are, or how this particular debility is produced and operates, are queſtions which appear to me highly intereſting and curious. I have therefore been at conſiderable pains to examine and arrange a very accurate and extenſive regiſtry in ſuch a manner as I hope will throw ſome light on theſe queſtions. As it is to the accuracy of modern regiſters that we are originally indebted for our knowledge of the facts in queſtion, I apprehend, it is from the ſame ſource only that we ſhall be enabled ſatisfactorily to explain them.

Of the regiſtry incloſed, I beg leave to obſerve to you, Sir, that it has been kept from its commencement by a man of uncommon accuracy (one of the under-clerks of our Houſe of Commons); and that as the poor women and their children are obliged to paſs through his office, before leaving the Hoſpital, his ſituation is ſuch that there is no likelihood of his being deceived. It exhibits to our view the occurrences of 28 years in above 20,000 inſtances: a number which I am inclined to think can hardly appear inſufficient for eſtabliſhing ſome general inferences and concluſions on a tolerably ſure foundation. Although my reaſoning on theſe matters ſhould not appear very concluſive, or my calculations perfectly accurate, yet I flatter myſelf, that the facts will neither be unacceptable nor uſeleſs to you.

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* Vol. II. p. 247.

I believe it may be ſafely aſſerted, that anatomy has not hitherto detected any internal difference between the animal œconomy of the male and female, which can be ſuppoſed to account for their difference of mortality, more eſpecially in early Infancy; and this (it deſerves to be particularly remarked) is the period during which the chances are much the greateſt againſt male life. It is a matter of common obſervation that *males*, *cæteris paribus*, grow to a greater ſize than females, both *in utero* and every ſubſequent period of their growth. Conſequently, they muſt meet with more difficulty, and endure more hardſhip and fatigue, in the hour of birth. Accordingly, practitioners in midwifry, taught by experience, know, that when any conſiderable difficulty occurs in the birth of a child (for example, in all the different kinds of preternatural labours) they ſtand a much better chance of ſaving the life of a female than of a male. It is on this principle we can explain what our regiſtry concurs with others in proving, *viz.* that near one-half more males than females are ſtill-born. Naturaliſts are agreed, that the head of the human fœtus is larger in proportion to its body than that of any other animal; and I believe it is certain, that no animal whatever brings forth its young with ſo much difficulty, pain, and danger, as a woman. Now as we know that the head contains one of the moſt important organs of the body to life, it is highly reaſonable to ſuppoſe, that any additional injury which it ſuſtains in delivery may produce very material effects on the whole ſyſtem. Theſe effects though often may not be always immediate. They may operate in weakening the male conſtitution ſo as to render it more apt to be affected by any exciting cauſe of diſeaſe ſoon after birth, and leſs able to ſtruggle againſt it. It may be aſked, how this will apply to the difference of mortality in great towns and country ſituations? The anſwer evidently is, that in great towns rickets, ſcrophula, and other diſeaſes affecting the bones, and producing conſequent mal-conformation of the female ſex, are more frequent than in healthy country ſituations.

There is another circumſtance, Sir, which may have ſome influence in producing that particular *debility* which you mention. It is this: as the ſtamina of the male are naturally conſtituted to grow to a greater ſize, a greater ſupply of nouriſhment *in utero* will be neceſſary to his growth than to that of a female. Defects in this particular, proceeding from delicacy of conſtitution or diſeaſes of the mother, muſt of courſe be more injurious to the male ſex. And although the male children may be ſo lucky as to eſcape abortion and the perils of delivery, it is probable, that they will be more apt to languiſh under diſeaſe, or die at ſome future period, from the application of noxious cauſes to an originally half-ſtarved frame. To a perſon little accuſtomed to conſider phyſiological ſubjects, this reaſoning may appear ſomewhat obſcure. It may, perhaps, be ſomewhat illuſtrated by conſidering that nouriſhment of the fœtus *after* birth which nature has provided for. Suppoſe every mother in a great city obliged to ſuckle and nurſe her own child, *without* the aſſiſtance of ſpoon-meat; and every mother in the adjacent country to do the ſame. Of the former there would not perhaps be one *good* nurſe in *five*; and of the latter, perhaps, *not one* bad in *ten*. The difference of mortality that would enſue both to mothers and children thus ſituated, and the greater ſufferings of the male than female ſex, may be eaſily conceived, but not eaſily calculated. We ſee that, when a woman conceives twins, and has two fœtuſes *in utero* to nouriſh inſtead of one, it becomes peculiarly fatal both to her and her offspring. The chances are above four to one greater againſt her than againſt a woman bringing forth one child, and about two to one againſt her iſſue *.

Give me leave, Sir, to call your attention a little further to the facts relating to twins. They are ſingular and curious, at the ſame time that they ſerve to confirm ſome of the preceding reaſoning. Near *one-half* more twins die, and near *one-third* more are ſtill-born, than of ſingle children. And why?—It is not becauſe they meet with greater difficulties in the birth. On the contrary, it is a known fact, that, being much leſs than other children, women bring them forth with more eaſe. Does it not then proceed from a ſcanty nutrition, by which they are oftener blighted *in utero* than ſingle children; and, when born alive, have leſs ſtrength to ſupport life through the firſt ſtages of its exiſtence.

It is farther worthy of obſervation, that though *double* the numbers of twins die and are ſtill-born, compared to ſingle children, yet the proportion of male twins loſt to females is *leſs*. Only one-fifth more of the male ſex die than of the female, and only one-third more is ſtill-born. Whereas of ſingle children, whoſe proportional mortality is one-half leſs, *one-fourth* more of the male ſex die, and near double the number is ſtill-born. To what then are we to attribute this leſſened mortality in favour of male twins? Probably to their brain and nervous ſyſtem ſuffering leſs during delivery, on account of their heads being much ſmaller than thoſe of ſingle children. Were I diſpoſed to be prolix, I could offer many more plauſible arguments on this ſubject; but to you, Sir, I am ſure they would be unneceſſary. There is only one circumſtance remaining, relative to the proportion of the ſexes, which I cannot paſs over in ſilence. We ſee evident wiſdom in the creation of a greater number of males than females; but why the proportion they bear to each other differs in different countries and ſituations, and why there ſhould be a ſeventeenth more males born of ſingle children than twins, are queſtions which I leave to be decided by thoſe philoſophers who underſtand the theory of generation better than I do. Be this as it may, I am convinced that the majority in favour of the male ſex is ſooner deſtroyed than the generality of writers ſeem to be aware of. Did the limits of this letter permit, I think, I could prove from Dr. SHORT's own data*, that the majority of males is deſtroyed long before the common marriageable period; but I ſhall content myſelf with an obſervation or two on the regiſtry before us. If one-half of the whole born in this hoſpital die before three years, which is the eſtabliſhed computation for great cities; and if, on the loſs of ſomewhat more than a *third* of this half, a majority of 1177 be reduced to 483 by a loſs of 694, as appears from the regiſtry, it is pretty evident, that by the death of the two remaining thirds, a majority will be left in favour of the female ſex. It is obvious, that the ſtatement with regard to twins corroborates this ſuppoſition; for of them, inſtead of a fifth, there is near one *half* dead and ſtill-born, the conſequence of which is, that we ſend out a majority of females. It may be objected, that their males do not bear ſo great a proportion to the females; and that, therefore, it is not to be expected they ſhould keep up their majority ſo long. But there is only a ſeventeenth fewer males produced; whereas it has been already ſhewn, that there is a much greater proportion between the deaths of ſingle and twin males againſt the former and in favour of the latter.

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* Compare the 7th and 14th, 6th and 13th inferences in the annexed extracts.

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* New Obſervations, p. 72. et ſeq.

Such are the outlines, Sir, of my ſentiments on this ſubject. I have aſſumed the liberty of addreſſing them to you without ceremony, as a well-wiſher to every member of the republic of letters. I ſhall be happy, ſhould your ſentiments happen to coincide with mine, or if I can be of any farther ſervice in promoting your very laudable inquiries.

I am, Sir, with great reſpect, &c.

JOSEPH CLARKE.

Lying-in Hoſpital,
June 9, 1785.

Dr. CLARKE's *ſecond Letter to the Rev. Dr.* PRICE.

SIR, Dublin, Oct. 22, 1785.

ENCOURAGED by your approbation of my former letter, I will take the liberty of ſtating to you a few more facts and obſervations, which I hope you will judge an Appendix to it of ſome importance.

With the view of aſcertaining how far ſome of the foregoing conjectures are well founded, and of determining with greater preciſion the more obvious differences between the male and female ſex in infancy, I began in the month of July laſt by weighing forty children, twenty of each ſex, and by taking the dimenſions of their heads. In the months of Auguſt and September I repeated the ſame experiment twice, taking ſuch children as appeared to have arrived at the full period of geſtation promiſcuouſly as they happened to be born.

I weighed them all a few hours after birth, before they had taken food, and before purgative medicines had time to operate. For this purpoſe, I made uſe of a ſmall ſpring or pocket ſteelyard, which weighs any thing (not heavier than a few pounds) appended to it with ſufficient accuracy. To this was attached a flannel bag, into which the children were put, at firſt, naked; but this I ſoon found very troubleſome. The nurſes often wanted time ſufficient to aſſiſt me, and timid mothers were afraid of their infants catching cold; I was therefore obliged to weigh them with their cloaths on, and to ſubtract a certain quantity from the groſs weight of each child, according as it was full, middling, or light cloathed. Whatever inaccuracy this may have introduced, as to the real weight of the children, it can but little influence their comparative weights, or the differences between the two ſexes, which it was my object to aſcertain.

For meaſuring their heads, I made uſe of a piece of painted or varniſhed linen tape, divided into inches, halves, and quarters. The varniſh has the good effect of preventing the length of ſuch a meaſure being readily affected by variations in the humidity of the atmoſphere, &c.; and it has little or no elaſticity. In this part of the experiment then I can pretend to conſiderable accuracy. I took firſt the greateſt circumference of the head from the moſt prominent part of the occiput around over the frontal ſinuſes; and, ſecondly, the tranſverſe dimenſion from the ſuperior and anterior part of one ear, acroſs the fontanelle, to a ſimilar part of the oppoſite ear. Theſe dimenſions appeared to me the moſt likely to afford data for determining the reſpective ſizes of the brain in the different ſexes. The reſult was as follows:

Twenty males.			Twenty females.		
Weight. lbs. &c.	Circumference of heads. Inches.	Dimenſions from ear to ear. Inches.	Weight. lbs. &c.	Circumf. of heads. Inches.	Dimen. from ear to ear. Inches.
Experiment 1.					
149½	282	152	137¼	272	143
Experiment 2.					
144½	277	146¼	135	272	147
Experiment 3.					
148	280	147½	132	273	143¼
Totals.					
442	839	445¾	404¼	817	433¼
Average weight, &c.					
7 lbs. 5 oz. 7 dr.	14	7¼	6 lbs. 11 oz. 6 dr.	13⅝	7²⁄₉

Having found the relative proportions between the ſexes to turn out thrice with ſo much uniformity, and obſerving them to correſpond pretty nearly with ſome experiments, made for very different purpoſes by the late Profeſſor ROEDERER, of Gottingen, I did not think it neceſſary to proſecute the ſubject farther.

Upon the whole, it may be obſerved, that the difference of weight between the male and female at birth may be rated at about nine ounces, or nearly a twelfth part of the original weight. In the circumference of their heads there is a difference of near half an inch, or about a 28th or 30th part; and the ſame proportion of a 28th is pretty nearly preſerved in the tranſverſe dimenſion. It is evident, as the bony paſſage

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through which infants paſs is of a certain determined capacity, that, were their heads equally incompreſſible with thoſe of adults, the difference of half an inch in their ſize would often prove fatal to them. By the compreſſibility of their heads, however, in *well formed* women, this difficulty is by time ſurmounted. The effects which ſuch a compreſſion on the *brain* may produce, have not hitherto been well attended to.

In reckoning children, weighing from 5½ to 6½, 6 pounds weight, and from 6½ to 7½, 7, and ſo forth, in order to avoid fractions, I find the numbers of males and females, arranged according to their weight, to ſtand as follow.

Males.								Females.							
lbs.	4	5	6	7	8	9	10	lbs.	4	5	6	7	8	9	10
N°	0	3	6	32	16	2	1	N°	2	9	14	25	8	2	0

Hence it appears, that the majority of males runs thus: ſeven, eight, ſix, five; whilſt that of the females is ſeven, ſix, five, eight. Hence alſo appears the merciful diſpenſations of Providence towards the female ſex; for when deviations from the medium ſtandard occur, it is remarkable, that they are much more frequently below than above this ſtandard. In 120 inſtances there are only five children exceeding eight pounds and a half in weight. The ſame may be obſerved with regard to the ſize of their heads. Only ſix meaſured above 14½ inches in circumference, and theſe all of the male ſex; five meaſured 14¾, and one 15. In tranſverſe dimenſions only four exceeded 7¾, the largeſt of which was 8½; whereas deviations under the ſtandard in theſe particulars were very numerous, never however under 12 around and 6¼ acroſs.

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In the year 1753, Dr. ROEDERER publiſhed a Paper, *De pondere et longitudine Infantum recens natorum*, in the Commentaries of the Royal Society of Gottingen, of which the celebrated HALLER was the principal inſtitutor, and long the preſident. In this Paper he proves, in the cleareſt manner, by inconteſtible experiments, the abſurdity of the ideas of obſtetric writers with regard to the progreſs of the ovum during geſtation, and the weight of the fœtus after birth. He ſhews, although they ſtate the weight of the fœtus, come to the full time, to be from 12 to 14 or 16 pounds, that it is more generally 6 or 7, and very rarely exceeds eight. This deſerves particular notice for two reaſons; firſt, becauſe it ſerves to ſhew how little dependence is to be placed on the aſſertions of authors who copy each other ſervilely, without having recourſe to experiment even in the moſt obvious caſes; and, ſecondly, becauſe this paper has been overlooked by ſome of the moſt celebrated writers and teachers of midwifry now living. What idea are we to form of the accuracy of one of our lateſt ſyſtematic writers, who (telling us that he has been a practitioner of midwifry, in a capital city, for twenty years, and a teacher for more than twelve) ſtates, in one page of his work, that the weight of a fœtus at eight months is about ſeven pounds; and on the oppoſite page, that at full time it weighs from twelve to fourteen pounds *?

Of 27 children, carried to the full period of geſtation, weighed and meaſured in length by ROEDERER, without any attention to the difference of ſex, I find, that 18 were of the male and 9 of the female ſex; and that the average weight of the former was about 6 lbs. 9 oz., that of the latter about 6 lbs. 2 oz. 2 dr. Whether he and I uſed the ſame weights, I cannot exactly ſay. He obſerves, that he uſed the civil pound of Gottingen, which I can eaſily perceive conſiſted of 16 ounces, as mine did; but whether a German ounce be the ſame with ours, I have not *data* to determine. The average length of the males meaſured by him is about $20\frac{1}{3}$ inches, and of the females about $19\frac{17}{18}$. He weighed alſo the placentæ of 21 lying-in women, 16 of whom had borne male children, and five female. The average weight of the former was 1 lb. $2\frac{1}{4}$ oz.; that of the latter 1 lb. 2 oz. Hence it appears, that in other circumſtances, beſides thoſe I have taken notice of, the male and female ſex differ. So far I thought it neceſſary to take extracts from Dr. Roederer's paper, as his obſervations and mine throw light on each other, and add confirmation to both.

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* See a Treatiſe of Midwifry (p, 88. and 89.) diveſted of *technical* terms and *abſtruſe theories*, by A. HAMILTON, M. D. 8° edit. London, 1781.

The limits of this letter will not permit me, Sir, to treſpaſs much farther on your patience. There is one circumſtance or two ſo intimately connected with my former letter, that I cannot paſs them over in ſilence. Having found that males ſuffer more in the birth than females, I was deſirous of knowing whether the chance of the mother's recovery was thereby in any degree affected; and to determine this I was once more at the pains of turning over our regiſtry with care. I found, that of 214 women, dead of ſingle children, 50 were delivered of ſtill-born males, and 15 of ſtill-born females; 76 of living males, and 73 of living females. Of the 15 dead of twins, 6 had twins one of each ſex; 6 others had twins both of the male ſex; and three had twins both of the female ſex. All of which twins (two or three excepted), it is very remarkable, ſurvived the death of their mothers. It would appear then, that the life of the mother is principally endangered in thoſe caſes where the bulk of the male's head precludes the poſſibility of his being brought into the world alive, either by the efforts of nature or art. The conception of twins we have obſerved to be more fatal to the mother than that of ſingle children. The average weight of 12 twins, which have occurred to me of late, I find to be 11 lbs. a pair. The largeſt pair weighed 13 lbs. and the leaſt 8½. From ſome rude attempts made to aſcertain the weight of the contents of the gravid uterus in caſes of twin and ſingle children, I am inclined to think, that they are to each other as about 15 to 10, or perhaps 14½ to 9½.

Believe me, Sir, with great reſpect, &c.

J. CLARKE.

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An Abſtract of the Regiſtry kept at the Lying-in Hoſpital, in Dublin, from the 8

31ſt of December, 1784. By B. H. Regiſter.

		Number of Patients admitted	Went out not delivered	Delivered in the Hoſpital.	Boys born	Girls born.	Total number of children.
From 8th to 31ſt of December,	1757	55	– –	55	30	25	55
	1758	455	1	454	255	207	462
	1759	413	7	406	228	192	420
	1760	571	15	556	300	260	560
	1761	537	16	521	283	249	532
	1762	550	17	533	279	266	545
	1763	519	31	488	274	224	498
	1764	610	22	588	287	308	595

as *one* to about *twenty*.

women having twins, as *one* to about *ſixty*.

women dying in child-bed, as *one* to about *eighty-ſeven*.

n the year 1757 to 1784.

ɔarous, Twins, Triplets, &c.

Children.

Sex.		Dead.		Still-born.	
M.	F.	M.	F.	M.	F.
342	320	116	91	29	20
320		91		20	
662		207		49	
		49			

Total 256 dead and ſtill-born.

twins to females born . .	17 to 16
dying under 16 days . .	1 to $3\frac{1}{5}$
ſtill-born . . .	1 to $13\frac{1}{2}$
twins dying to females . .	5 to 4
—— ſtill-born to ditto . .	3 to 2
ɔorn and dead of each ſex to the whole	1 to $2\frac{1}{5}$
ɜn dying . . .	1 to 22

Totals of twins, &c. dead and ſtill-born.

	Males.	Females.
	116	91
	29	20
	145	111
Born	342	320
Dead and ſtill-born	145	111
Sent out living	197	209
		197

XVIII. *Some Particulars of the preſent State of* Mount Veſuvius; *with the Account of a Journey into the Province of* Abruzzo, *and a Voyage to the Iſland of* Ponza. *In a Letter from Sir* William Hamilton, *K. B. F. R. S. and A. S. to Sir* Joſeph Banks, *Bart. P. R. S.*

Read May 4, 1786.

SIR, Naples, January 24, 1786.

THE eruption of Mount Veſuvius, which began in the month of November 1784, nearly at the moment of my return from England to this Capital, and which continued in ſome degree till about the 20th of laſt month, has afforded much amuſement to travellers unacquainted with this wonderful operation of nature, but no new circumſtance that could juſtify my troubling you with a letter on the ſubject. The lava either overflowed the rim of the crater, or iſſued from ſmall fiſſures on its borders, on that ſide which faces the mountain of Somma, and ran more or leſs in one, and at times in three or four channels, regularly formed, down the flanks of the conical part of the volcano; ſometimes deſcending and ſpreading itſelf in the valley between the two mountains; and once, when the eruption was in its greateſt force, in the month of November laſt, the lava deſcended ſtill lower, and did ſome damage to the vineyards, and cultivated parts at the foot of Veſuvius, towards the village of St. Sebaſtiano; but generally the lava, not being abundant, ſtopped and cooled before it was able to reach the valley. By the accumulation of theſe lava's on the flanks of Veſuvius, its form has been greatly altered; and by the frequent exploſion of ſcoriæ and aſhes, a conſiderable mountain has been formed within the crater, which now riſing much above its rim has likewiſe given that part of the mountain a new appearance. Juſt before I left Naples, in May 1783, I was at the top of Veſuvius. The crater was certainly then more than 250 feet deep, and was impracticable, its ſides being nearly perpendicular. This eruption, however, has been as ſatisfactory as could be deſired by the inhabitants of this city, a prodigious quantity of lava having been diſgorged; which matter, confined within the bowels of the earth, would probably have occaſioned tremors; and even ſlight ones might prove fatal to Naples, whoſe houſes are, in general, very high, ill built, and a great number in almoſt every ſtreet already ſupported by props, having either ſuffered by former earthquakes, or from the looſe volcanic ſoil's having been waſhed from under their foundations by the torrents of rain water from the high grounds which ſurround Naples, and on which a great part of the town itſelf is built.

From the time of the laſt formidable eruption of Mount Veſuvius, in Auguſt 1779 (deſcribed in one of my former communications to the Royal Society) to this day, I have, with the aſſiſtance of the Father Antonio Piaggi [*], kept an exact diary of the operations of Veſuvius, with drawings, ſhewing, by the quantity of ſmoke, the degrees of fermentation of the volcano; alſo the courſe of the lava's during this laſt eruption, and the changes that have been made in the form of the mountain itſelf by the lava's and ſcoriæ that have been ejected. This journal is becoming very curious and intereſting; it is remarkably ſo with reſpect to the pointing out a variety of ſingular effects that different currents of air have upon the ſmoke that iſſues from the crater of Veſuvius, elevated (as you know, Sir) more than 3600 feet above the level of the ſea; but, except the ſmoke increaſing conſiderably and conſtantly when the ſea is agitated, and the wind blows from that quarter, the operations of Veſuvius appear to be very capricious and uncertain. One day there will be the appearance of a violent fermentation, and the next all is calmed again: but whenever the ſmoke has been attended with conſiderable ejections of ſcoriæ and cinders, I have conſtantly obſerved, that the lava has ſoon after made its appearance, either by boiling over the crater, or forcing its paſſage through crevices in the conical part of the volcano. As long as I remain in this country, and have the neceſſary aſſiſtance of the above-mentioned ingenious Monk (who is as excellent a draughtſman as he is an accurate and diligent obſerver) the Veſuvian diary ſhall be continued; and I hope one day to have the honour of preſenting theſe curious manuſcripts (which begin now to be voluminous) to the Royal Society, if it ſhould think them worthy of a place in the Library of the Society.

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[*] This Padre Antonio Piaggi is the ingenious Monk who invented the method of unfolding and recovering the burnt ancient manuſcripts of Herculaneum, and who reſides conſtantly at Reſina, at the foot, and in full view, of Mount Veſuvius.

Having never had an opportunity of examining the iſlands of Ponza, Palmarole, Zannone, and other ſmall iſlands, or rather rocks, ſituated between the iſland of Ventotiene and Monte Circello, near Terracina, on the Continent; and thinking that by a tour of theſe iſlands I ſhould be enabled to render my former obſervations more complete, and to communicate to you, Sir, ſome account of the only volcanic parts of this neighbourhood hitherto undeſcribed, I determined to take advantage of the abſence of their Sicilian Majeſties (who were then making the tour of Italy) and viſit theſe iſlands. But before I put this plan in execution, I made a long excurſion in the province of Abruzzo, as far as the Lake of Celano, anciently called Fucinus, and where the famous Emiſſary of the Emperor CLAUDIUS (a moſt ſtupendous work * for draining that lake) remains nearly entire, though filled up with rubbiſh and earth in many parts, and of courſe uſeleſs. The water of this lake, which is more than 30 miles in circumference, increaſes daily, and is deſtroying the rich and cultivated plains on its borders. It is ſurrounded by very high mountains, many of them covered with ſnow, and at the foot of them are many villages, and rich and well cultivated farms. Upon the whole it is the moſt beautiful lake I ever ſaw, and would be complete, if the neighbouring mountains were better wooded. This lake furniſhes abundance of fiſh, but not of the beſt quality: a few large trout, but moſtly tench, barbel, and dace. In the ſhallow water on the borders of the lake, I ſaw thouſands of water ſnakes, purſuing and preying upon a little fiſh like our thornbacks, but much better armed, though their defenſive weapons ſeemed to avail them but little againſt ſuch ravenous foes.

I went with torches into the emiſſary of CLAUDIUS as far as I could. It is a covered under-ground canal, three miles long, and great part of it cut through a hard rock; the other parts ſupported by maſonry, with wells ſunk to give air and light.

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* A deſcription of this emiſſary of CLAUDIUS, with plans (though not very exact) has been publiſhed by FABRETTI, in the ſame book in which he has given an account of TRAJAN's column.

According to SUETONIUS, CLAUDIUS employed thirty thouſand men eleven years on this great work, intended to convey the ſuperfluous water of the lake into the bed of the river Liris, now called Garigliano; and I make no doubt, but that if it was cleared and repaired, it would again anſwer that purpoſe.

In its preſent ſtate it is a moſt magnificent monument of antiquity.

The whole country from Arpino, the native place of MARIUS*, by Iſola, Sora, Civitella, and Capiſtrello, to the lake of Celano, is, in my opinion, infinitely more beautiful and picturesque than any ſpot I have yet ſeen on the Alps, in Savoy, Switzerland, or the Tyrol. The road is not paſſable for carriages, and indeed is ſcarcely ſo, even in ſummer, for horſes or mules, and is often infeſted with banditti; a party of which, conſiſting of twenty-two, had quartered themſelves in a village which I paſſed through, and left it but a week before my arrival. There are many wolves and ſome bears in the adjacent mountains, which alſo commit their depredations in the winter. The tyger-cat, *gatto pardo*, or lynx, is ſometimes found in the woods of this part of Abruzzo.

The road follows the windings of the Garigliano, which is here a beautiful clear trout ſtream, with a great variety of caſcades and water-falls, particularly a double one at Iſola, near which place CICERO had a villa, and there are ſtill ſome remains of it, though converted to a chapel. The valley is extenſive, and rich with fruit trees, corn, vines, and olives. Large tracts of land are here and there covered with woods of oak and cheſtnut, all timber trees of the largeſt ſize. The mountains neareſt the valley riſe gently, and are adorned with either modern caſtles, towns, and villages, or the ruins of ancient ones. The next range of mountains, riſing behind theſe, are covered with pines, larches, and ſuch trees and ſhrubs as uſually abound in a like ſituation: and above them a third range of mountains and rocks, being the moſt elevated part of the Apennine, riſe much higher, and, being covered with eternal ſnow, make a beautiful contraſt with the rich valley above-mentioned; and the ſnow is at ſo great a diſtance, as not to give that uncomfortable chill to the air, which I have always found in the narrow vallies of the Alps and the Tyrol. Excuſe me, Sir, if from the impreſſion which this enchanting and little frequented country has left on my mind, I have been led to depart from the ſubject of this letter, to which I will return directly.

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* MARIUS had a large villa, about twelve miles diſtant from Arpino. I went to viſit the ſpot, on which now ſtands the only convent of the auſtere order of La Trappe in Italy. It is in the Pope's ſtate, and has been evidently built of the ruins of MARIUS's houſe, and its preſent name is *Caſa Mari*.

On the 15th of Auguſt laſt I went in a felucca to the iſland of Iſchia. I have nothing to add to my former obſervations on this iſland, already communicated to the Royal Society; except that about ſixty yards from the ſhore, at a place called St. Angelo, ſituated between the towns of Iſchia and Furia, a column of boiling water bubbles upon the ſurface of the ſea with great force, and communicates its heat to the water of the ſea near it; but as the wind was very high, and the ſurf conſiderable, I was not able then to examine this curious ſpot as I could have wiſhed, but will return there on purpoſe ſome other time. The inhabitants of the neighbourhood told me, that it always boiled up in the ſame manner, winter and ſummer; and that it was of great uſe to them in bending their planks for ſhip-building; and that the fiſhermen alſo frequently made uſe of this natural cauldron to boil their fiſh. Though I have paſſed at different times many weeks in the iſland of Iſchia, I never before heard of this phænomenon; but in my deſcription of this iſland mention is made of ſeveral ſpots where, near the ſhore, I had found, when bathing in the ſea, the ſand under my feet ſo hot as to oblige me to retire haſtily. This boiling ſpring reminds me of one near Viterbo in the Roman State, which I have ſeen, and is called the Bulicame. It is a circular pool of about ſixty feet in diameter, and exceedingly deep, the water of which is conſtantly boiling. It is ſituated in a plain ſurrounded by volcanic mountains. A ſtony concretion floats on the ſurface of the pool, which being carried off by the ſuperfluous water is depoſited, and is conſtantly forming a labes or tuffa, of which all the ſoil around the pool is compoſed. You have ſeen, Sir, the like operation in greater perfection in Iceland, at the famous boiling ſpring of Geyſer. I am convinced, that many of the finer ſort and moſt compact tuffa's we meet with, in countries formed by volcanoes, have been produced in the ſame manner.

The 18th of Auguſt I arrived at the iſland of Ventotiene, about twenty-five miles from Iſchia. It is greatly improved ſince my former viſit, ſeven or eight years ago, when his Sicilian Majeſty firſt planted a little colony there. It then produced neither corn nor wine; now it furniſhes annually at leaſt ſeventy butts of wine and two thouſand *tomoli* of corn. The ſoil is remarkably fertile, from whence it probably took its ancient Greek name of Pandataria. This iſland contains at preſent more than three hundred inhabitants. The iſland of Ventotiene, and the ſmaller one called St. Stefano, within a mile of it, having been deſcribed in my Campi Phlegræi, as being both entirely compoſed of volcanic matter, I need not trouble you further on their ſubject; I will only mention a curious circumſtance in the natural hiſtory of birds, of which I was informed by an officer of the garriſon of Ventotiene, who is a great ſportſman, and ſhoots often in the iſland of St. Stefano, inhabited only by hawks, and a large kind of ſea-gulls; but is occaſionally viſited, as a reſting place, by divers ſorts of birds of paſſage. In the month of May great flights of quails arrive there from Africa, ſpent with fatigue; and many of them fall an eaſy prey for the hawks and ſea-gulls; but, as their arrival depends upon one prevailing wind, there is often an interval of many days between one flight and another. My informer aſſured me, that the hawks conſtantly, during the flights, make a proviſion of each day's prey, laying them up in ſeparate heaps of ſix or ſeven near their haunts, always feeding firſt upon thoſe of the oldeſt date. The ſea-gulls have not the ſame foreſight, but greedily fall upon their unhappy victims in their languid ſtate before they reach the ſhore, and, having beat them down into the ſea, ſwallow numbers of them whole. Extraordinary as this may appear, yet as facts related by perſons of credibility in any branch of natural hiſtory are always pleaſing, I thought you would excuſe this digreſſion. Give me leave likewiſe to add, for the information of the curious in antiquities, that, during my ſtay in the iſland of Ventotiene, I got out of the ruins of an elegant ancient bath (ſuppoſed to have been built for the uſe of JULIA, daughter of AUGUSTUS, whilſt ſhe was in exile here) a fragment of a tile, on which are ſtamped the following characters in baſſo relievo,

HACINI
IVLIAI
AVGVS. F

which, according to the interpretation of a celebrated antiquary at Naples, mean *Opus* HACINI *ad commodum Balnei* JULIÆ AUGUSTÆ *factum*. I was informed, that ſeveral entire tiles, with a like inſcription, had been dug up on the ſame ſpot, and had been made uſe of in building the church and barracks newly erected in this iſland. Another fragment of a tile was likewiſe found here, and given to me, with the following inſcription:

SAB. A PI.

which the ſame antiquary explains, SABINAE AVGVSTAE, Piæ Imperatrici *dicatum Balneum*; but, I believe, there is no mention in ancient authors of SABINA having been at Pandataria: of JULIA's baniſhment to this iſland there can be no doubt.

Between Ventotiene and the iſland of Ponza, and from the latter at the diſtance of about twelve miles, a group of rocks riſe ſeveral feet above the ſurface of the ſea. They are called the *Botte*, and are compoſed of a compact lava; probably they are the ſmall remains of another volcanic iſland, the ſofter parts of which may have been carried off and levelled by the action of the ſea, which is open and violent here.

The 20th of Auguſt I arrived at the iſland of Ponza, about thirty miles from Ventotiene, and the next day I went round it in my boat. It is near five miles long: its greateſt breadth not more than half a mile, and in ſome parts not more than five hundred feet. It is ſurrounded by innumerable detached rocks, ſome of them very high, and moſt of which are of lava; in many are regularly formed baſaltes, but none in large columns. In ſome parts the baſaltes have a reddiſh tint of iron ochre, are very ſmall, and irregularly laid one over another. Some maſſes of them are in a perpendicular, others in an horizontal, and others again in an inclined poſition: and the rocks themſelves, in which theſe maſſes are found, are lava of the ſame nature as the baſaltes. At firſt ſight theſe rocks have very much the appearance of the ruins of ancient Roman brick or rather tile buildings, as may be ſeen in the drawing (ſee Tab. XI. fig. 1.) taken on the ſpot. One rock, as appears in the drawing (ſee Tab. XII. fig. 4.) is compoſed of large ſpherical baſaltes; and in many parts of the iſland I found the lava had inclined to take the like ſpherical form, though on a much ſmaller ſcale, ſome of the firſt mentioned round baſaltes being near two feet in diameter. All theſe rocks have certainly been detached by the action of the ſea from the iſland, which is intirely compoſed of volcanic matter, lava's, and tuffa's, of various qualities and tints, green, yellow, black, and white. Some of the tuffa's, as well as the lava's, are of a texture more compact than others; and in ſome parts of the iſland great tracts ſeem to have undergone the ſame operation as is mentioned in one of my former communications to be in full force at a ſpot called the Piſciarelli, on the outſide of the Solfaterra, near Puzzole, and where a hot ſulphureous vitriolic acid vapour converts all which it penetrates, whether lava's, tuffa's, volcanic aſhes, or pumice ſtones, into a pure clay, moſtly white, or with a light tint of red, blue, green, or yellow. The appearance of a tract of volcanic country, which has undergone this operation, is well expreſſed in the view of the inſide of the harbour of Ponza (Tab. XI. fig. 2.). But I was ſo ſtruck with the beautiful and uncommon appearance of one of theſe high volcanic grounds converted to a pure light-coloured clay (Tab. XII. fig. 1.) in contraſt with a neighbouring dark baſaltic rock, that I cauſed the drawing, which accompanies this letter (ſee Tab. XII.) to be made on the ſpot. You, Sir, who have ſeen ſuch a variety of countries, will ſtill think this view ſingular and beautiful. I can aſſure you, it is very exact, except the rock of round baſaltes (fig. 4.) which, in nature, is at a diſtance from this ſpot, and only placed here to illuſtrate what I have written on its ſubject. In one part of the iſland there is a ſort of tuffa, remarkably good for the purpoſe of building. It is as hard as our Bath ſtone, and nearly of the ſame colour, without any mixture of fragments of lava or pumice ſtone, which uſually abound in the tuffa's in the neighbourhood of Naples, Baia, and Puzzole.

The drawing (ſee Tab. XI.), which is a view of the harbour of Ponza, will give you a very good idea of the appearance of the iſolated rocks of lava and baſaltes which have been ſeparated, by the force of the ſea, from the ſofter parts of the iſland, and of which there are an infinite number, as you will ſee in the exact geometrical plan of the iſland of Ponza (Tab. X.), which likewiſe accompanies this letter.

When I was laſt in England, I inquired of many of the manufacturers of glaſs, whether it had ever happened, that the glaſs cooling in their furnaces had taken any diſtinct forms like priſms or cryſtallizations; but I got no ſatisfactory anſwer until I applied to the ingenious Mr. PARKER, of Fleet-ſtreet, who not only informed me, that, ſome years ago, a quantity of his flint glaſs had been rendered unſerviceable by taking ſuch a form in cooling; but alſo gave me ſeveral curious ſpecimens of the glaſs itſelf: ſome of them are in laminæ, which may be eaſily ſeparated; and others reſemble baſaltic columns in miniature, having regular faces. I was much pleaſed with this diſcovery, proving to me, beyond a doubt, the volcanic origin of moſt baſaltes. Many of the rocks of lava of the iſland of Ponza are, with reſpect to their configurations, ſtrikingly like the ſpecimens of Mr. PARKER's above-mentioned glaſs, none being very regularly formed baſaltes, but all having a tendency towards it. Mr. PARKER could not account for the accident that occaſioned his glaſs to take the baſaltic forms; but I have remarked, both in Sicily and at Naples, that ſuch lava's as have run into the ſea, are either formed into regular baſaltes, or have a great tendency towards ſuch a form. The lava's of Mount Etna, which ran into the ſea near Iacci, as appears in my account of them in the Campi Phlegræi, are perfect baſaltes; and a lava that ran into the ſea from Mount Veſuvius, near Torre del Greco, in 1631, has an evident tendency to the baſaltic forms. On Mount Veſuvius I never found any thing like columns of baſaltes, except the abovementioned at Torre del Greco, and ſome fragments of very complete ones, which I picked up near the crater, after the eruption of 1779, and which had been thrown out of the mouth of the volcano.

The iſland of Palmarole, which is about four miles from Ponza, is not much more than a mile in circumference, is compoſed of the ſame volcanic matter, and probably was once a part of Ponza; and indeed it appears as if the iſland of Zannone, which lies at about the ſame diſtance from the iſland of Ponza, was once likewiſe a part of the ſame iſland of Ponza; for many rocks of lava riſe above water in a line between the two laſt mentioned iſlands, and the water is much ſhallower there than in the other parts of the gulph of Terracina.

The iſland of Zannone is larger and much higher than Palmarole, and the half of the iſland neareſt the Continent is compoſed of a lime-ſtone, exactly ſimilar to that of the Apennines, on the Continent near it; the other half is compoſed of lava's and tuffa's, reſembling in every reſpect the ſoil of the other iſlands juſt deſcribed. Neither Palmarole nor Zannone are inhabited; but the latter furniſhes bruſhwood in abundance for the uſe of the inhabitants of Ponza, whoſe number, including the garriſon, amounts to near ſeventeen hundred.

The uninhabited iſland of St. Stefano furniſhes fuel in the like manner for the inhabitants of Ventotiene.

It is probable, that all theſe iſlands and rocks may in time be levelled by the action of the ſea. Ponza, in its preſent ſtate, is the mere ſkeleton of a volcanic iſland, as little more than its harder vitrified parts remain, and they ſeem to be ſlowly and gradually mouldering away. Other new volcanic iſlands may likewiſe be produced in theſe parts.

The gulphs of Gaeta and Terracina may, in the courſe of time, become another Campo Felice *; for, as has been mentioned in one of my former communications on this ſubject, the rich and fertile plain ſo called, which extends from the bay of Naples to the Apennines, behind Caſerta and Capua, has evidently been intirely formed by a ſucceſſion of ſuch volcanic eruptions. Veſuvius, the Solfaterra, and the high volcanic ground, on which great part of this city is built, were once probably iſlands; and we may conceive, the iſlands of Procita, Iſchia, Ventotiene, Palmarole, Ponza, and Zannone, to be the outline of a new portion of land, intended by nature to be added to the neighbouring Continent; and the Lipari iſlands (all of which are volcanic) may be looked upon in the ſame light with reſpect to a future intended addition of territory to the iſland of Sicily. If you caſt your eye, Sir, on the map at the head of my deſcription of the Campi Phlegræi, you will better underſtand my meaning.

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* The governor of the caſtle of Ponza, who has reſided there fifty-three years, told me, that the iſland was ſtill ſubject to earthquakes; that there had been one violent ſhock there about four years ago; but that the moſt violent one he ever felt there was on the very day and hour of the great earthquake which deſtroyed Liſbon; that two houſes out of three, which were then on the iſland, were thrown down. This ſeems to prove, that the volcanic matter, which gave birth to theſe iſlands, is not exhauſted.

The more opportunities I have of examining this volcanic country, the more I am convinced of the truth of what I have already ventured to advance, which is, that volcanoes ſhould be conſidered in a creative rather than a deſtructive light. Many new diſcoveries have been made of late years, particularly, as you well know, Sir, in the South-Seas, of iſlands which owe their birth to volcanic exploſions; and ſome, indeed, where the volcanic fire ſtill operates. I am led to believe, that upon further examination, moſt of the elevated iſlands at a conſiderable diſtance from Continents would be found to have a volcanic origin; as the low and flat iſlands appear in general to have been formed of the ſpoils of ſea productions, ſuch as corals, madrepores, &c. But I will ſtop here, and not deviate from the plan which I have hitherto ſtrictly followed, of reporting faithfully to my learned Brethren of the Royal Society ſuch facts only as come immediately under my own obſervation, and as I think may be worthy of their notice, and leave them at full liberty to reaſon upon them.

We may flatter ourſelves, as a very great progreſs has been made of late years in the knowledge of volcanoes, that by combining ſuch obſervations as we are already in poſſeſſion of, with thoſe which may be made hereafter, in the four quarters of the world (in all of which nature ſeems to have operated in a like manner), a much better theory of the earth may be eſtabliſhed than the miſerable ones that have hitherto appeared.

Thoſe who have not had an opportunity of examining a volcanic country, as I have for more than twenty years, would little ſuſpect, that many curious productions and combinations of lava's and tuffa's were of a volcanic origin; eſpecially when they have undergone various chemical operations of nature, ſome of which, as I have mentioned in a former communication, as well as in this, have been capable of converting tuffa's, lava's, and pumice ſtone, into the pureſt clay.

I have remarked, that young obſervers in this branch of natural hiſtory are but too apt to fall into the dangerous error of limiting the order of nature to their confined ideas: for example, ſhould they ſuſpect a mountain to have been a volcano, they immediately climb to its ſummit to ſeek for the crater, and if they neither find one, or any ſigns of lava or pumice-ſtone, directly conclude ſuch a mountain not to be volcanic: whereas, only ſuppoſe Mount Etna to have ceaſed erupting for many ages, and that half of its conical part ſhould have mouldered away by time (which would naturally be the conſequence) and the harder parts remain in points, forming an immenſe circuit of mountains (Etna extending at its baſis more than one hundred and fifty miles); ſuch an obſerver as I have juſt mentioned would certainly not find a crater on the top of any of theſe mountains, and his ideas would be too limited to conceive, that this whole range of mountains were only part of what once conſtituted a complete cone and crater of a volcano. It cannot be too ſtrongly recommended to obſervers in this, as well as in every other branch of natural hiſtory, not to be over-haſty in their deciſions, nor to attribute every production they meet with to a ſingle operation of nature, when perhaps it has undergone various, of which I have given examples in the iſland which has been the principal ſubject of this letter. That which was one day in a calcareous ſtate, and formed by an inſect in the ſea, becomes vitrified in another, by the action of the volcanic fire, and the addition of ſome natural ingredients, ſuch as ſea ſalts and weeds, and is again transformed to a pure clay by another curious proceſs of nature. The naturaliſt may indeed decide as to the preſent quality of any natural production; but it would be preſumption in him to decide as to its former ſtates. As far as I can judge in this curious country, active nature ſeems to be conſtantly employed in compoſing, decompoſing, and recompoſing; but ſurely for all-wiſe and benevolent purpoſes, though on a ſcale perhaps much too great and extenſive for our weak and limited comprehenſion.

I have the honour to be, with great regard and eſteem, &c.

W. HAMILTON.

POSTSCRIPT.

THE earth is not yet ſo perfectly quiet in Calabria and at Meſſina, as to encourage the inhabitants to begin to rebuild their houſes, and they continue to live in wooden barracks. There has, however, been no earthquake of conſequence during theſe laſt three months. My conjecture, that the volcanic matter (which was ſuppoſed to have occaſioned the late earthquakes) had vented itſelf at the bottom of the ſea between Calabria and Sicily, ſeems to have been verified; for the pilot of one of his Sicilian Majeſty's ſciabecques, having ſome time after the earthquakes caſt anchor off the point of Palizzi, where he had often anchored in twenty-five fathom water, found no bottom till he came to ſixty-five fathom, and having ſounded for two miles out at ſea towards the point of Spartivento in Calabria, he ſtill found the ſame conſiderable alteration in the depth of the ſea. The inhabitants of Palizzi likewiſe declare, that during the great earthquake of the 5th of February, 1783, the ſea had frothed and boiled up tremendouſly off their point.

EXPLANATION OF THE PLATES.

Tab. X. Plan of the iſland of Ponza.

Tab. XI. View of part of the inſide of the harbour of the iſland of Ponza.

Fig. 1. Rock of lava, which in many parts is formed into regular ſmall baſaltes of a reddiſh caſt, having probably been tinged with ſome ochre. Moſt of the detached rocks of this iſland reſemble this.

Fig. 2. See p. 374.

Tab. XII. View taken from the outſide of the harbour of the iſland of Ponza, near the Lighthouſe.

Fig. 1. Rock of volcanic matter converted to pure clay.

— 2. Ditto, with ſtrata of pumice-ſtone.

— 3. Rocks of lava inclining to take baſaltic forms.

— 4. Rock compoſed of ſpherical baſaltes.

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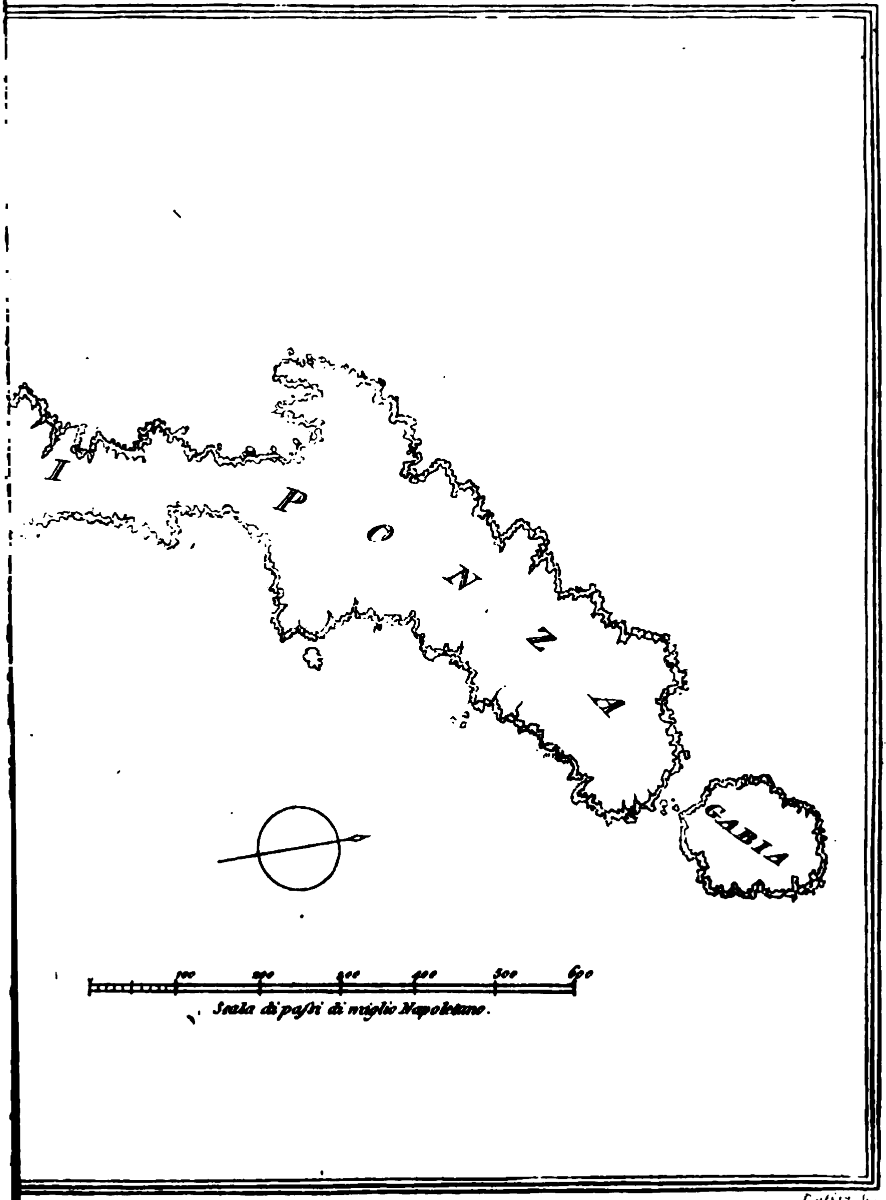
XIX. *An Account of a new Electrical Fiſh. In a Letter from Lieutenant* William Paterſon *to Sir* Joſeph Banks, *Bart. P. R. S.*

Read May 11, 1786.

SIR,

WHILE at the iſland of Johanna, one of the Comora iſlands, in my way to the Eaſt-Indies, with the 98th Regiment, I met with an electrical fiſh, which has hitherto eſcaped the obſervation of naturaliſts, and ſeems in many reſpects to differ from the electrical fiſhes already deſcribed; which induces me to ſend you the following account of it, with a very imperfect Drawing, and to beg that, if you think it deſerves attention, you will do me the honour of preſenting it to the Royal Society. The ſituation of a ſubaltern officer, in an army upon foreign ſervice, will, I hope, ſufficiently apologize for my ſending you ſo very imperfect a ſketch of the fiſh, which was made in the field, in a hot climate, under every diſadvantage.

The fiſh is ſeven inches long, two inches and a half broad, has a long projecting mouth, and ſeems to be of the genus Tetrodon. The back of the fiſh is a dark brown colour, the belly part of ſea-green, the ſides yellow, and the fins and tail of a ſandy green. The body is interſperſed with red, green, and white ſpots, the white ones particularly bright; the eyes large, the iris red, its outer edge tinged with yellow. (See Tab. XIII.)



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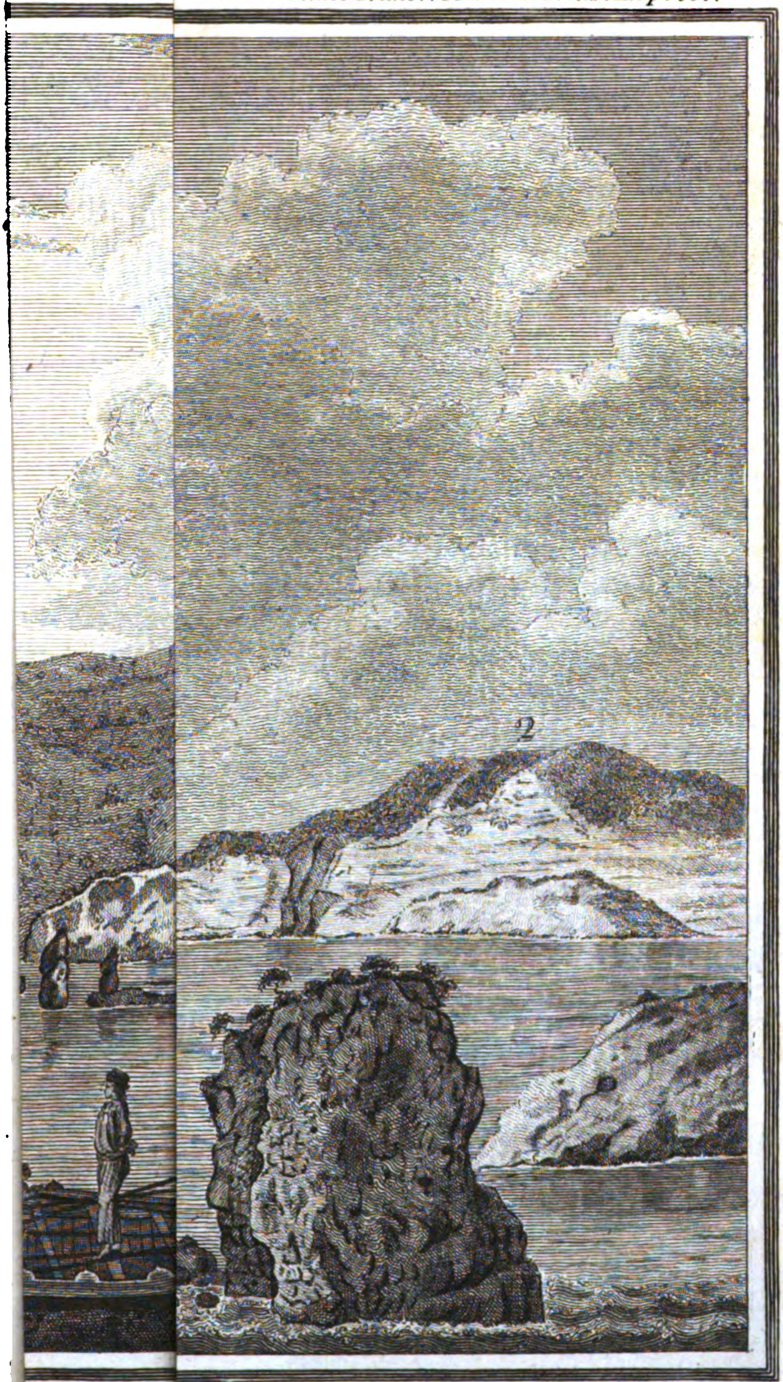
Philos. Trans. Vol. LXXVI. Tab. XI. *p.* 380.



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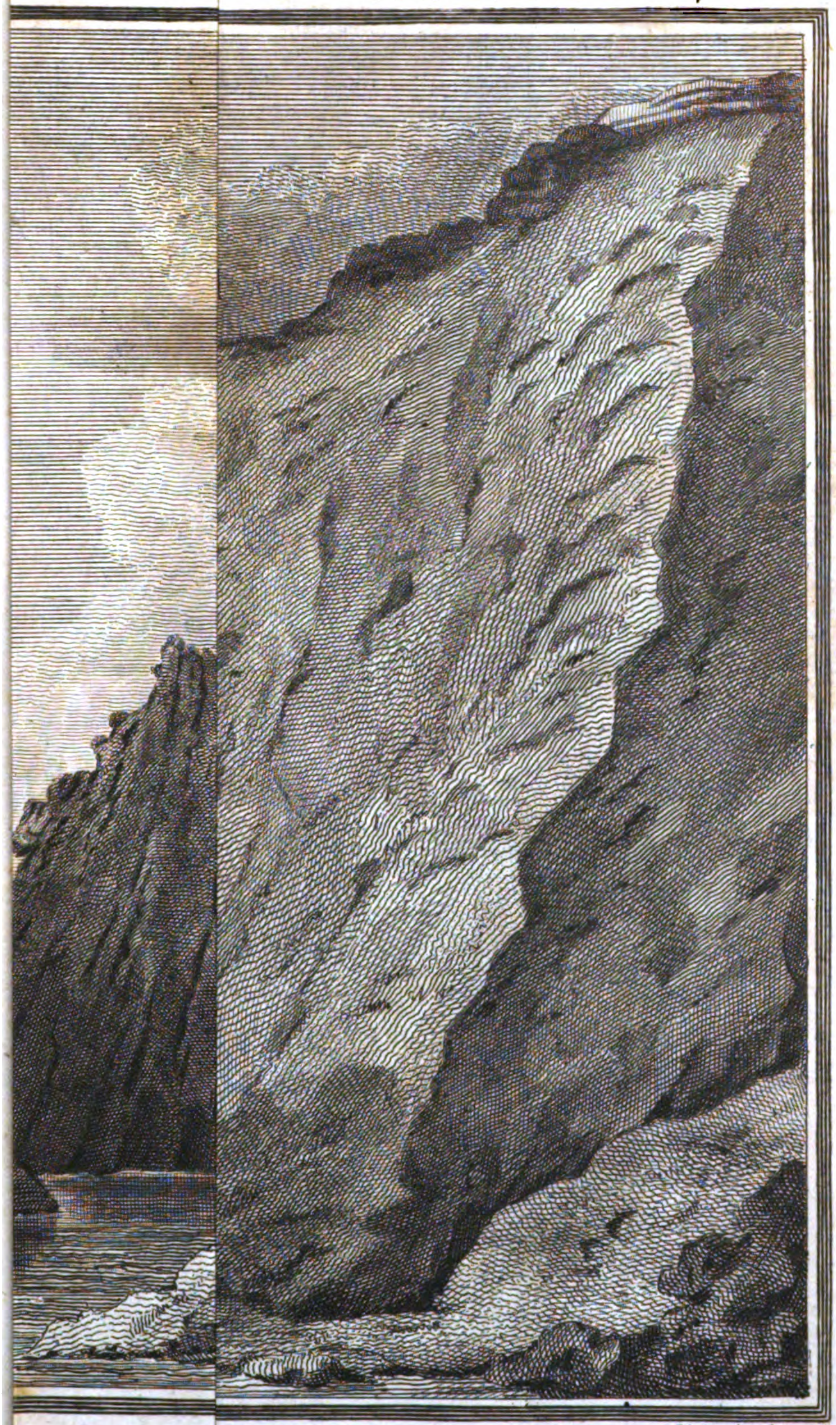

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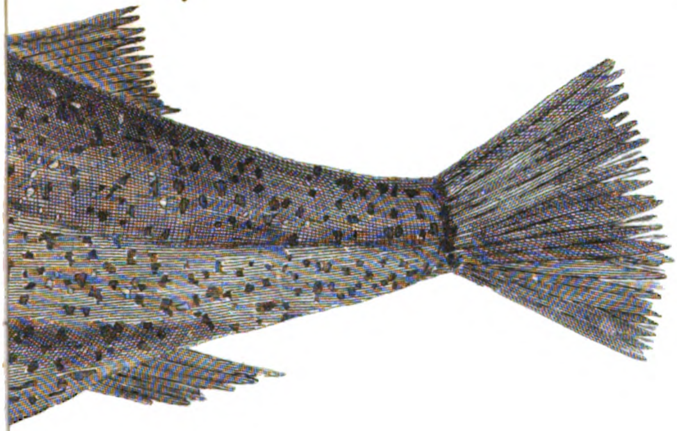

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The iſland of Johanna is ſituated in latitude 12° 13′ ſouth. The coaſt is wholly compoſed of coral rocks, which are in many places hollowed by the ſea. In theſe cavities I found ſeveral of the electrical fiſhes. The water is about 56° or 60° of heat of FAHRENHEIT's thermometer. I caught two of them in a linen bag, cloſed up at one end, and open at the other. In attempting to take one of them in my hand, it gave me ſo ſevere an electrical ſhock, that I was obliged to quit my hold. I however ſecured them both in the linen bag, and carried them to the camp, which was about two miles diſtant. Upon my arrival there, one of them was found to be dead, and the other in a very weak ſtate, which made me anxious to prove, by the evidence of others, that it poſſeſſed the powers of electricity, while it was yet alive. I had it put into a tub of water, and deſired the Surgeon of the regiment to lay hold of it between his hands; upon doing which he received an evident electrical ſtroke. Afterwards the Adjutant touched it with his finger upon the back, and felt a very ſlight ſhock, but ſufficiently ſtrong to aſcertain the fact.

After ſo very imperfect an account, I will not trouble you with any obſervations of my own upon this ſingular fiſh; but beg you will conſider this only as a direction to others who may hereafter viſit that iſland, and from their ſituation, and knowledge in natural hiſtory, may be better able to deſcribe the fiſh, and give an account of its electrical organs.

I have the honour to be, with great eſteem, &c.

W. PATERSON,
Lieutenant 98th regiment.

* * *

XX. *Obſervation of the Tranſit of Mercury over the Sun's Diſc, made at* Louvain, *in the* Netherlands, *May 3,* 1786. *By* Nathaniel Pigott, *Eſq. F. R. S.*

Read June 15, 1786.

Louvain, May 15, 1786.

THE tranſit of Mercury was to happen a few days after my arrival at this place from England. Although I brought no aſtronomical inſtruments with me, I wiſhed to obſerve this phænomenon; and upon application to M. THYSBAERT, *Préſident du Collège Royal,* a very diſtinguiſhed Member of this Univerſity, he ſupplied me, in the politeſt manner, with the following inſtruments, and a convenient place for the obſervation. He carried his attention to the moſt trifling circumſtances, in order to make my ſituation, in every reſpect, agreeable. The inſtruments he provided me with were a Gregorian reflector of 21 inches focal length, with an aperture of 4½ inches, the magnifying power of which I eſteemed about 70 or 80, with a good quadrant 18 inches radius, and a compound pendulum clock, ſteadily fixed, beating dead ſeconds. Theſe inſtruments were made in London, and uſed for the obſervation of Mercury. The rate of the clock, and the apparent times thence deduced, were obtained by equal altitudes of the ſun, taken with the quadrant. Theſe were the only inſtruments I had, and therefore ſuch obſervations as are not dependent on the *meaſure of time*, are to be conſidered as made by eſtimation; however, the moſt important, the internal and external contacts of Mercury, and hence the egreſs of his center and the interval of time between the two contacts, were made in a very ſatisfactory manner. About ſix o'clock, when I attended for the obſervation, there being a great number of ſolar ſpots, Mercury might eaſily have been miſtaken for one; but his motion ſoon removed every doubt in that reſpect. Flying clouds obſcured the ſun at intervals; but during the laſt half hour, the weather was fine, the ſky clear, the limb of the ſun well defined; Mercury round and very black. There ſeems to have been ſome miſtake, in reſpect of this phænomenon, either in the calculation or the printing of the *Connoiſſance des Temps* of this year: the emerſion of the center of Mercury is there ſet down at 19 h. 45′ apparent time at Paris; whereas, by my obſervation, the egreſs of the center at Louvain was at 20 h. 47′ 28″ or 29″ apparent time. Taking here no other equation into conſideration, except the difference of meridians between Paris and Louvain, which, by a great number of obſervations, I determined in 1775 to be 9′ 37″ in time *, the emerſion of the center at *Paris* muſt have been at 20 h. 37′ 51″ or 52″, which differs nearly 53′ from the computed time. By the ſame reaſoning, I ſhould ſuppoſe, that the emerſion of the center of Mercury at Greenwich was obſerved at 20 h. 28′ 35″ or 36″. Mercury being ſo very near the earth, the effects of parallax muſt be conſiderable, and the weſtern ſituation of Greenwich of 18′ 53″ in time from this place, muſt occaſion a retardation, which, on computation, may be hereafter allowed for, and added to the ſuppoſed time of the egreſs above-mentioned, deduced from my obſervation here.

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* See Philoſophical Tranſactions, vol. LXVIII. p. 654.

While I am writing this Paper, the reſpective ſituation of Greenwich and Louvain ſtrikes me. The latitude of Greenwich is 51° 28′ 40″, that of Louvain 50° 53′ 3″ *; the difference little more than half a degree. Greenwich is 9′ 16″ weſt, and Louvain 9′ 37″ eaſt of the Paris Obſervatory; the parallax above-mentioned is therefore nearly, but in a contrary ſenſe, equal at the two places, and thus the effects of both are compenſated relatively to Paris. What other advantage may reſult from this circumſtance, would require conſideration. I have not leiſure, at preſent, to revolve it in my mind, as I am deſirous to lay this Paper before the Royal Society as ſoon as I can, by the favour of Dr. MASKELYNE, our Aſtronomer Royal.

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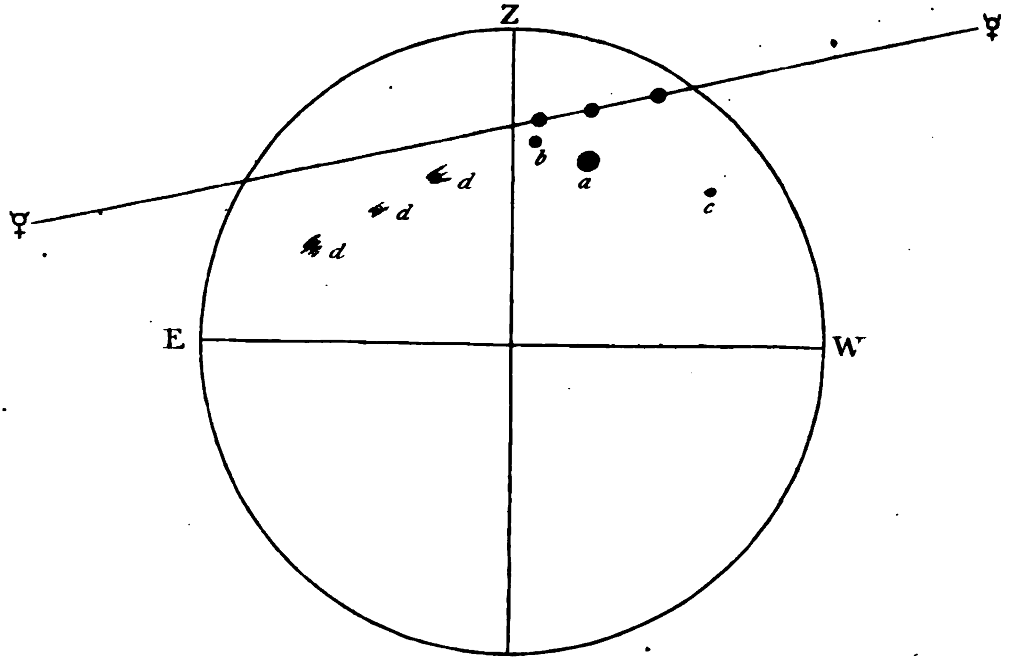
* See Philoſophical Tranſactions, vol. LXVIII. p. 643.

Obſervations of the Tranſit of Mercury at Louvain.



Apparent time.
H. M. S.
18 32 30 flying clouds; Mercury ill defined, with ſome twiſting.
19 13 30 the ſpot (*a*) appears thrice as large as Mercury; ſpot (*b*) twice ditto.
19 16 30 a perpendicular from the ſun's limb on E. W. biſects Mercury and (*b*).
19 27 30 perpendicular, as above, equi-diſtant from (*a*) and (*b*).

Apparent time.			
H.	M.	S.	
19	34	30	perpendicular, as above, biſects Mercury and ſpot (*a*).
19	42	30	perpendicular from Mercury on E. W. is beyond ſpot (*a*).
19	45	30	it is ſenſibly beyond ſpot (*a*).
20	12	30	perpendicular from the ſun's limb on E. W. equidiſtant from (*a*) and (*c*).
20	27	30	Mercury very black, round and well defined.
20	45	41	internal contact; perhaps a few ſeconds too ſoon.
20	47	26	emerſion of center by *eſtimation*.
20	49	16	external contact.
20	49	41	Mercury certainly clear of the ſun.

a. b. c, d. d. d. are ſpots in the ſun; Z. zenith; E. Eaſt; W. weſt of the ſolar diſc.

The internal contact being at 20 h. 45′ 41″, and the external at 20 h. 49′ 16″, the emerſion of the center of Mercury muſt have been at 20 h. 47′ 28″½; which differs only 2½ ſeconds from the eſtimated time; and the duration of total egreſs was 3 m. 35 ſ.

N. B. The reaſons why the nine firſt obſervations are all marked at 30″ is, that in reality they were ſet down at the *minute* only; and that I have added 2′ 30″ to each to reduce the time by the clock to apparent time; more nicety would have been ſuperfluous: but the *four laſt* were rigorouſly computed.

⁂

XXI. *Obſervation of the late Tranſit of Mercury over the Sun, obſerved by* Edward Pigott, *Eſq. at* Louvain *in the* Netherlands; *communicated by him in a Letter to* Nevil Maſkelyne, *D. D. F. R. S. and Aſtronomer Royal.*

Read June 15, 1786.

SIR, Louvain, May 27, 1786.

WE have been fortunate here in ſeeing Mercury's egreſs. I obſerved it thus:

Apparent time, May 3.

H.	M.	S.	
20	45	25	Mercury's limb in contact with the ſun's limb; uncertain.
20	45	37	ditto . ditto; certain.
20	47	17	Mercury biſected by the ſun's limb.
20	49	22	Mercury quite out; clouds for a ſhort interval, which renders the obſervation rather doubtful.

Though the air was not perfectly free from thin clouds, nevertheleſs the limbs were well defined. At 20h. 45′ 25″, when I firſt judged Mercury's limb in contact with the ſun's, his form, I think, became rather oval. Theſe obſervations were made with RAMSDEN's two-feet achromatic, magnifying about 70 times. The above times diſagreeing ſo conſiderably with the tables will, I imagine, not a little ſurpriſe M. DE LA LANDE.

I remain, Sir, with great regard, &c.

EDW. PIGOTT.

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XXII. *Additional Obſervations on making a Thermometer for meaſuring the higher Degrees of Heat.* *By Mr.* Joſiah Wedgwood, *F. R. S. and Potter to Her Majeſty.*

Read June 22, 1786.

MY thermometer for meaſuring the higher degrees of heat having been honoured with the notice of this illuſtrious Society, I now requeſt a further indulgence for a few more obſervations on the ſame ſubject.

In my firſt Paper * I communicated every thing that experience had then taught me, reſpecting both the conſtruction and uſe of this thermometer; but more extenſive practice has ſince convinced me, that other managements and precautions are neceſſary, in order to bring it to the perfection it is capable of receiving: for pieces made of the ſame clay, and exactly of the ſame dimenſions, have been found to differ in the degree of their diminution by fire, in conſequence of ſome circumſtances in the mode of their formation, at that time unheeded, and very difficult to be developed.

Of the two ways propoſed for forming them, the mould and the preſs, the former was made choice of, as being, for general uſe, the moſt commodious. The ſoft clay was preſſed into a ſquare mould with the fingers; and the pieces, when dry, were pared down on two oppoſite ſides, by means of a paring gage made for that purpoſe, ſo as to paſs exactly to 0° at the entrance of the converging canal of the meaſuring gage.

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* Philoſophical Tranſactions, Vol. LXXII.

But the pieces thus formed have been found liable, in paſſing through ſtrong fire, to receive a little alteration in their figure, which produces an uncertainty with reſpect to their ſubſequent meaſurement: the two ſides, inſtead of continuing flat, become concave; the edges, both at top and bottom, projecting beyond the middle part, ſometimes very conſiderably, as at *a* and *b*, fig. 1, (Tab. XIV.) where AB repreſents a perpendicular ſection of an unburnt piece, and *a b* a like ſection of the ſame piece after it has undergone a heat of 160 degrees. This irregularity in the form, which is ſenſible only after paſſing through the high degrees of fire, was obſerved in ſome of the early experiments, but was not then looked upon as being productive of any error.

On more attentively examining this matter, it appeared, that when the clay is preſſed into a mould, the ſurface in contact with the mould acquires a more compact texture than the inner part of the maſs;—that this compactneſs reſtrains, in ſome degree, its diminution in the fire;—and therefore, that when this ſurface, or leſs diminiſhable cruſt, is pared off from the two ſides only, the piece may be conſidered as having its upper and lower ſtrata (AA and BB, fig. 1.) compoſed of a leſs diminiſhable matter than the intermediate part, the neceſſary conſequence of which ſtructure will be ſuch a figure as we find the pieces to aſſume; for if any ſtratum in the maſs ſhrinks leſs than the reſt, the extremities of that ſtratum muſt be left proportionably prominent. That this was the true cauſe of the inequality, I was convinced by firing ſome pieces *unadjuſted*, with all their ſurfaces entire, as they came from the mould; for theſe pieces, after paſſing through the ſame ſtrong fires with the preceding, continued flat, with the angles regularly ſharp, and without the leaſt ſenſible prominence in any part.

Some of the moulds, employed for this uſe, were made of plaſter, a material more convenient for the workman than metal, as the pieces part more freely from it, but which contributed greatly to increaſe the above-mentioned irregularity: for the plaſter, by abſorbing a portion of the water from the clay contiguous to it, renders the ſurface at the ſame time, even at the inſtant of contact, much more *conſiſtent*, and conſequently more difficult to preſs into the angles of the mould; ſo that the outſides of theſe pieces were not only more *compreſſed*, but formed of clay of a different *temper* from the inner parts, being much drier or firmer; a circumſtance which, as will appear hereafter, reſtrains ſtill more their diminution in the fire.

The moulds were therefore laid aſide, and the preſs adopted in their ſtead; for as the ſoft clay, preſſed in a cylindrical veſſel, gives way and eſcapes through an aperture made for that purpoſe (by which means it is formed into long rods), the ſides of the piece cannot be ſuppoſed to receive ſo great a degree of compreſſure againſt the ſides of the aperture through which it is *delivered* in this operation, as it does againſt the ſides of the mould, by which it is *confined* till every part has born a preſſure ſufficient to force the clay into every angle, which is much greater than even a workman would imagine till he comes to try the experiment himſelf.

But with this change ſome new difficulties aroſe; for pieces preſſed through the ſame aperture, and from the ſame preſſ-ful of clay, and adjuſted, when dry, to the ſame point in the gage, were found, after paſſing together through the ſame ſtrong fires, to differ in their dimenſions from one another, in ſome inſtances more than any of the preceding.

Having hitherto paid no particular attention myſelf to the mere manual labour of preſſing the clay, I determined, upon this event, to go through that and every other operation, however ſimple and ſeemingly inſignificant, with my own hands. In doing this I obſerved, that the power neceſſary for forcing the clay through an aperture which bore but a ſmall proportion to the diameter of the maſs of clay in the preſs, was ſo great as to ſqueeze out, along with the clay that firſt paſſed through, a conſiderable portion of the water that belonged to the reſt. From this over-proportion of water in the compoſition of the firſt pieces they were ſoft and ſpongy, and the ſucceeding ones more and more compact, till at length the clay proved ſo ſtiff as ſcarcely to be forced through at all.

Clay, containing different proportions of water, is well known to diminiſh differently in drying; but it was not imagined that, when dry, there would be any difference in its ſubſequent diminutions by fire. Experiments however, multiplied in a variety of circumſtances, ſhewed deciſively, what the pieces formed in the mould had given grounds to ſuſpect, that thoſe formed of the ſofteſt clay, and which had undergone the leaſt preſſure, diminiſhed moſt in burning; and that the diminution is uniformly leſs and leſs, in proportion to the greater degree of preſſure or compactneſs.

The knowledge of the cauſe of the irregularity ſuggeſted a remedy. I leſſened the width of the preſs very much, ſo as to bring the diameter of the maſs of clay, and that of the aperture through which it is delivered, to a nearer proportion with one another. A much leſs degree of force being now ſufficient, the pieces, or rods, were proportionably more uniform, though there was ſtill a ſenſible difference, in conſiſtence, between thoſe which were firſt and laſt preſſed out from the ſame maſs of clay. The intermediate ones, within a certain diſtance from the two extremes, correſponded very nearly with one another; ſo that by rejecting a ſufficient number of the firſt and laſt, and uſing the *intermediate ones only*, the inequality may be conſidered as almoſt annihilated.

I nevertheleſs ſtill found that, in ſtrong fire, the edges became a little prominent, though not ſo much as before. I was aware that theſe pieces muſt partake, in ſome degree, of the imperfection of thoſe made in the mould; having their ſurfaces rendered, by their friction againſt the ſides of the aperture, more compact than the inner part. But I ſuſpected that ſomething might depend alſo upon the *form*, and accordingly made many trials for aſcertaining the form that might be leaſt liable to this irregularity: the angles only were bevilled off, the ſides were rounded, the pieces were rounded all over, made of ovals and other curves, and both the longeſt and ſhorteſt dimenſions were uſed as the extent to be meaſured: the general reſult was, that the nearer they came to a circular figure, the leſs inequality they contracted in the fire, and by making them entirely circular, the imperfection appeared to be obviated altogether; cylindric pieces bearing the ſtrongeſt fires without the leaſt appearance of prominence or inequality in any part of their ſurface. I have therefore choſen this laſt form, leaving only one narrow flat ſide (*ab*, fig. 2.) as a bottom for the pieces to reſt upon, and to diſtinguiſh the poſition in which they are to be meaſured in the gage.

I have endeavoured at the ſame time to obviate whatever inaccuracy the inequality of compactneſs may be capable of producing, by ſo adjuſting the aperture through which the rods are preſſed, and on which their figure and dimenſions depend, as to ſuperſede the uſe of the paring gage altogether; that the whole ſurface may remain of the ſame uniform compactneſs which it received in the preſs. And as it is ſcarcely practicable, in any mode of forming ſoft clay, to have all the pieces preciſely of the ſame dimenſions after drying, I do not reject thoſe which come within two or three degrees of the ſtandard, but, inſtead of injuring the ſurface by paring or rubbing, I mark on the ends the degrees which they reſpectively exceed or fall ſhort; which degrees are accordingly to be ſubſtracted, or added, in all obſervations of heat made with thoſe pieces. Strictly ſpeaking, an allowance ought to be made alſo for the proportional diminution upon this exceſs or deficiency itſelf; but the allowance for three degrees would not, at the melting heats of copper, ſilver, or gold, amount to more than a ſeventh part of a degree; and at the extreme point of heat that I have been able to attain, when the piece has diminiſhed $\frac{160}{600}$, or nearly one-fourth of its whole thickneſs, it comes only to four-fifths of a degree.

It may be proper to take notice of an irregularity in the *apparent* diminutions of the pieces, which was ſometimes obſerved to happen from another cauſe, their bending a little in ſtrong fire, ſo as to be prevented from going ſo far in the gage as they would have done if they had continued perfectly ſtraight. But as this takes place only in pieces of conſiderable length, and as they derive no advantage of any kind from that length, the remedy is too obvious to need being here mentioned.

Another fallacious appearance aroſe, not from any imperfection in the pieces themſelves, but from a deception with reſpect to the heat in which the compariſon of them had been made. In one period of the courſe of my experiments, I employed, for firing them, a ſmall, ſhallow, cylindrical veſſel (fig. 3.) ſetting the pieces on end, cloſe together, on its bottom, and placing it in the middle of the fuel, in a common air-furnace, with care to keep the fire as equal all round it as poſſible. It was expected, that all the pieces would receive an equal heat; and as they were found, after the operation, to differ in their dimenſions, ſometimes conſiderably, from one another, theſe differences proved a ſource of much perplexity, till it was diſcovered that the pieces had really undergone unequal degrees of heat.

In the paper on the compariſon of this thermometer with FAHRENHEIT'S *, I have taken notice of the great difficulty of obtaining, in ſmall furnaces, a perfectly equal heat, even through the extent occupied by a few of theſe little pieces: and how different the heat may be in different parts of one veſſel, we may be ſatisfied by an eaſy experiment, *viz.* ſetting a cylindrical rod of clay, of the length of eight or ten inches, upright in the middle of a crucible, and urging it with ſtrong fire in a common ſmall furnace; the rod will be found very differently diminiſhed at different parts of its height; and if its height be ſufficient to reach ſome way above the fuel, nearly the whole range of the thermometric ſcale may be produced in one rod; an ocular proof, not only of the diverſity of heat within a ſmall compaſs, but likewiſe of the *peculiar* ſenſibility of this thermometer, every *part* of the maſs expreſſing diſtinctly the degree of heat which it has itſelf felt. It will be proper, in this experiment, to have a tube fixed in the bottom of the crucible, for keeping the rod ſteady, as at fig. 4. By this means the heat of my air-furnace renders a rod of the thermometric clay tapering, from about four parts in diameter at top to three at bottom, which are nearly the proportions between the width of the piece when unburnt, or but juſt ignited, and when it has ſuffered a heat of 160 degrees.

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* Philoſophical Tranſactions, vol. LXXIV.

To the foregoing ſources of inequality in the pieces, one more may be added, ſmall cavities, or air-bubbles accidentally incloſed, which ſometimes happened in the earlier experiments. In order to prevent theſe, particular attention is now paid by the workmen to what we call *banding* or *ſlapping* the clay, an operation by which its different parts are intermixed, and the maſs rendered of an uniform temper throughout. The workman takes a lump of the clay in his hands, perhaps of two pounds weight, and, breaking it in two in the middle, lays one part upon the other, and preſſes them flat again, repeating this forty or fifty times, or perhaps oftener. Now, conſidering the pieces at firſt as two diſſimilar maſſes, with any number of air-bubbles incloſed; each of theſe pieces being by the firſt doubling divided into two, by the next into four, by the third into eight, and ſo on in geometrical progreſſion, each of the original maſſes will be divided by the fiftieth repetition into upwards of eleven thouſand millions of millions of inviſible laminæ:—inviſible, becauſe the lump of clay would, long before the laſt doubling, be of one uniform colour, though at firſt one-half of it had been black, and the other white. If therefore no air be incloſed between the pieces at the times of their being put together in this proceſs, all the air which might have been in the maſs before would certainly be driven out; and, to avoid as much as poſſible the introduction of any freſh portions of air, the two ſeparated pieces are each time made ſmooth, and a little convex, on the ſurfaces that are to be brought together.

By due attention to the circumſtances above ſtated, any ſingle quantity of clay may be made up into thermometer-pieces, that ſhall differ very little, if any thing at all, from one another.

But a new difficulty now aroſe, more embarraſſing than any of the former; that of procuring freſh ſupplies of clay, of the ſame thermometric quality with the firſt. The quantity of the clay which, after trial of many others, I had made choice of, was ſmall; but the particular ſpot it was taken from being known, and having purchaſed the little eſtate in which it was raiſed, I had not a doubt of obtaining more of the ſame when it ſhould be wanted: for clays in general, when raiſed from an equal depth, in the ſame ſtratum, and near the ſame place, are found to poſſeſs the ſame properties, with reſpect to ductility in the hands of the workman, a diſpoſition to aſſume by fire a porcelain or vitreous texture, ſingly or in compoſition, and all other qualities relative to their uſe in pottery. In this, however, I was deceived; for when a freſh ſupply was wanted, to complete my experiments, though I had ſome taken from a pit joining to the firſt, and at the ſame depth, it was found to diminiſh differently from the former parcel. I then had ſome raiſed from different parts of the ſame field and bed, and at different depths; and in various other places in Cornwall, from the ſpot where this ſpecies of clay is firſt met with to the Land's-End; but all theſe clays differed ſo much from the firſt in the quantity of their diminution by fire, and moſt of them likewiſe from each other, that I deſpaired of being ever able to find one that would correſpond with it, or any natural clays that could be obtained twice of exactly the ſame *thermometric* properties, how ſimilar ſoever in other reſpects.

Upon a review of the numerous compariſons which I have made of theſe new clays, in different degrees of heat, from the commencement of redneſs up to intenſe fire, the moſt ſtriking differences of the greateſt part of them from the old ſeemed to originate in the lower ſtages of heat; and of thoſe which were got from the neighbourhood of the old, the variations from it in the higher ſtages ſeemed, for the moſt part, to be only conſequences of thoſe differences in the lower ones.

I have mentioned, in the firſt Paper, that the original thermometer-pieces had their bulk enlarged a little on the approach of ignition; but that by the time they became viſibly red-hot throughout, they are reduced to their former dimenſions again; and at this moment the thermometric diminution begins. The new clays had their bulk enlarged in a much greater proportion, and the enlargement was of much longer continuance: ſome of them required a heat of 15 degrees to deſtroy the increaſe which ignition had produced in their bulk, and bring them back to their original dimenſions: after this period, moſt of them diminiſhed pretty regularly, and uniformly with the old, being nearly ſo many degrees behind it, in all the ſucceeding ſtages of heat, as they required to bring them back from the enlarged ſtate.

I have mentioned alſo, in my former paper, that a quantity of air is extricated from the clay, moſt rapidly at the period in which the augmentation of bulk takes place; and that the augmentation was probably owing to this air forcing the particles of the clay a little aſunder, previous to the inſtant of its eſcape. It was therefore preſumed, that the greater extenſion of theſe new clays might be owing, either to a greater quantity, or ſtronger adheſion, of this combined air: and as clay, kept moiſt for a length of time, in certain circumſtances, undergoes a proceſs ſeemingly analogous to fermentation,

tion, it was hoped that, by ſuch a proceſs, part of its combined air might be detached.

But experiments made on this idea have proved, that theſe clays, kept moiſt for a twelvemonth,—kept for a conſiderable length of time in a heat juſt below viſible redneſs,—boiled in water for many hours,—alternately, and repeatedly, moiſtened and dried,—ſuffer no alteration in their thermometric properties, and continue to differ from the ſtandard clay juſt as much as they did at firſt.

Some of theſe new clays differed from the old in a property ſtill more eſſential, and by which I was much more diſconcerted; for though they continued diminiſhing with tolerable regularity, keeping only ſome degrees behind it, up to a certain period of heat, about that in which caſt iron melts; yet many of the pieces, urged with a heat known to be greater than that, were found not to be diminiſhed ſo much as thoſe which had ſuffered only that lower heat. Further experiments ſhewed, that, after diminiſhing to a certain point, they begin, upon an increaſe of the heat beyond that point, to ſwell again: and as this effect is conſtant in certain clays, and begins earlieſt in thoſe which are moſt vitreſcible, and as clays are found to ſwell upon the approach of vitrification, I look upon this ſecond enlargement of bulk, however inconſiderable, as a ſure indication of the clay or compoſition having gone beyond the true porcelain ſtate, and of a diſpoſition taking place towards vitrification; which ſtage is always, ſo far as my experience reaches, attended with a new extrication of air; and in ſome inſtances, this air being unable to make its eſcape from the tenacious maſs that envelopes it, the burnt clay is thereby ſo much increaſed in bulk as to ſwim on water like very light wood. The degree of heat therefore, at which this enlargement begins, may be conſidered as a criterion of the degree of vitreſcibility of the compoſition; which points out a new uſe of this thermometer, enabling us to aſcertain the *degree of vitreſcibility* of bodies that cannot actually be vitrified by any fires which our furnaces are capable of producing.

All my reſearches among the natural clays proving fruitleſs, and the experiments having ſhewn that all thoſe, which could ſufficiently reſiſt vitrification, diminiſhed *too little* in the fire, I endeavoured to find a body poſſeſſed of the oppoſite property, that is, diminiſhing *too much*, and, by a mixture of theſe two, to produce the *medium* diminution required. As I could not find any natural ſubſtance poſſeſſed of that property, which would not at the ſame time render the compound too vitreſcible, I was obliged to have recourſe to ſome artificial preparation; and as the earth of alum is the pure argillaceous earth, to which all clays owe their property of diminution in the fire, poſſeſſing that property in a greater or leſs degree according to the quantity of alum earth in their compoſition, I mixed ſome of this earth with the clay, and found it to anſwer my wiſhes completely, both in procuring the neceſſary degree of diminution, and increaſing its unvitreſcibility. So little is this compound diſpoſed to vitrification, that the greateſt heat I could give it, that of 160°, did not even bring it to a porcelain texture, but left it ſtill bibulous; and as it does not arrive at the *porcelain* ſtate in this fire, there can be no danger of its approaching too near to the *vitreſcent* in any heat that we can produce in a furnace.

In order to obtain the exact medium required, I took one of the beſt of the clays I had procured from Cornwall, and mixed it with different proportions of the alum earth, till the compoſition was found, on repeated trials, to agree with the original in all degrees of heat. This coincidence was not indeed eſſential; but as many degrees of heat were already before the public, meaſured by thermometer-pieces made of the firſt clay, and as the correſpondence of the firſt with FAHRENHEIT's ſcale had likewiſe been in ſome meaſure aſcertained, it was deſirable that the ſame degrees of heat ſhould continue to be expreſſed by the ſame numbers.

The alum earth is prepared for this purpoſe by diſſolving the alum in water, precipitating with a ſolution of fixed alkali, and waſhing the earth repeatedly with large quantities of boiling water: when the earth has ſettled, the water above it is let off by cocks in the ſides of the hogſheads; and when the veſſels are filled up with freſh water, care is taken to ſtir up the earth from the bottom, and mix it thoroughly with the liquor. I find it moſt convenient to uſe the earth undried, in its gelatinous ſtate, as in this ſtate it unites eaſily and perfectly with the clay; whereas, when the alum earth has concreted into dry maſſes, great labour is neceſſary to mix them uniformly together.

I have tried ſeveral different parcels of Engliſh alum, from the ſame and from different manufactories, and found no material difference in the quantity of earth it contains*. Nor indeed would it be of any conſeqence if there was a difference in this reſpect, as the proportion of alum earth neceſſary for different clays, and even for different parcels of the ſame clay, can only be aſcertained by repeated trials, adding ſucceſſive quantities of the earth till the deſired effect is found to be produced. Ten hundred weight of the Cornwall porcelain clay which I have now in uſe required all the earth that was afforded by five hundred weight of alum.

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* A difference in the quantity of earth *may* ariſe from different proportions of GLAUBER's ſalt and vitriolated tartar, of which I have found quantities very conſiderable, but nearly alike, in all the Engliſh alum I have examined. Theſe ſalts are doubtleſs formed by the kelp aſhes employed in the preparation of the alum. They are diſcovered by calcining the dried alum with charcoal powder, which decompoſes the alum only, leaving the other two ſalts intermixed with the alum earth, from whence they may be extracted by water.

It is material in this place to obſerve, that the earth of alum is extremely tenacious of water, inſomuch that, though apparently dry, the water and air amount to near as much as the pure earth, and are not to be completely driven out without a full red heat. When divided by the admixture of other earthy bodies, it parts with its water eaſier indeed than before; but a mixture containing ſo much of it as the thermometric compoſition does, is far more retentive of water than common clay, and requires to be kept for ſome time in a heat equal to that of boiling water, before it is to be conſidered as dry, that is, before the adjuſtment of the pieces in the gage. If they are adjuſted when only apparently dry, or of ſuch a degree of dryneſs as they can be brought to by a heat that the hand can bear, the heat of boiling water will diminiſh them two or three degrees; and the greateſt part of what they have thus been deprived of, they gradually recover again on being expoſed to the atmoſphere, ſo that the adjuſtment muſt be made immediately after the boiling heat.

By the ſame expedient to which I have thus been obliged to have recourſe for procuring to the porcelain clay of Cornwall the ſtandard degree of diminution, and reſiſtence to fire, the ſame qualities may probably be communicated to any other clay that is tolerably pure from calcareous earth and iron; ſo that the thermometer clay is no longer to be conſidered as the produce of any particular ſpot (which was the principal inconvenience originally imagined to attend it), but may be procured and prepared in all parts of the world where good common clay, and alum, are to be found; and *correſponding* thermometers may, conſequently, be conſtructed, without any ſtandard to copy from. For, if a converging canal be formed, of any convenient length, with the widths at the two ends in the proportion of 5 to 3, with the ſides perfectly ſtraight, and divided into 240 equal parts, numbering the diviſions from the wider end*;—and if a clay be obtained of ſuch quality, that when formed, in the manner already mentioned, into pieces of ſuch ſize as to enter to 0 in the gage or canal, theſe pieces ſhall juſt begin to diminiſh, or go a little further in the canal, by a heat viſibly red;—go to 27, by the heat in which copper melts;—about 90 by the welding heat of iron; about 160, by the greateſt heat that can be produced with coaked pit-coal in a well conſtructed common air-furnace, about eight inches ſquare, ſtill continuing bibulous, ſo as to ſtick to the tongue: ſuch gages, and pieces of ſuch clay, ſo adjuſted, will always compoſe correſpondent thermometers.

Having mentioned occaſionally ſeveral alternate periods of dilatation and contraction in clay, it may be proper to ſtate, and bring into one view, the whole ſucceſſion of changes which I have obſerved in this curious material; as otherwiſe they might create ſome confuſion in the minds of thoſe who have not had occaſion to think attentively on this ſubject, and lead them to aſk how a body ſo variable, and liable to ſuch oppoſite changes from different degrees of heat, can yet be a juſt meaſure of thoſe degrees.

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* Or the diviſions on the ſide may be continued to 300; and in that caſe, inſtead of the widths of the two ends being in proportion of the odd numbers 5 and 3, the one will be juſt double to the other.

The changes which take place in all the natural clays that have come under my examination are ſix.

1. The firſt is, the *ſhrinking* of the moiſt clay in drying, from the mere loſs of its water. The purer the clay is, the more water it requires to ſoften it, and the more it diminiſhes in bulk by the loſs of that water.

2. The dry clay, gradually heated, preſerves its bulk unvaried up to the approach of ignition. At this period it is *enlarged* a little; probably, as already obſerved, from its combined air endeavouring to eſcape.

3. When this air has made its eſcape, the clay begins to diminiſh, or to *loſe the bulk it had before acquired*; and returns back, ſooner or later, to the ſame dimenſions which it was of when dry. It is at this point that the thermometric diminution commences.

4. From this point the clay continues to *diminiſh* more and more in proportion as the heat is increaſed. This I call the *thermometric ſtage* of diminution: it is of greater or leſs extent, terminating at different periods of heat, according to the nature of the clay: in the ſtandard thermometer clay, it commences with viſible ignition, and continues to (doubtleſs far beyond) the extreme heats of our furnaces, an interval conſiſting of 160 degrees of the ſcale: in others, it begins 4, 6, and in ſome even 15 of thoſe degrees later, and terminates alſo much ſooner: and in ſome its whole extent is not above 20 of the ſame degrees. Throughout the greateſt part of this ſtage, the clays are found to retain their property of ſticking to the tongue and imbibing water: between this *bibulous* ſtate and the *vitreſcent* there is an intermediate one, diſtinguiſhed by the name of *porcelain*; and to the higher term of this porcelain ſtate the ſtage of thermometric diminution ſeems to continue.

5. When the clay has paſſed the porcelain ſtate, it begins to be *enlarged* again, a ſymptom of the vitreſcent ſtage being commenced; and in this period it ſwells more or leſs, according to the nature of its compoſition.

6. By further heat the ſwelled maſs, becoming fluid, ſubſides, is converted into glaſs or ſlag, and *contracted into leſs volume* than the clay occupied in any of its preceding ſtates.

It is plain, therefore, that clay can be a meaſure of heat no further than from ignition, or that point beyond ignition where the third ſtage terminates, to the beginning of the vitreſcent ſtage; and that, as the three firſt changes are completely paſſed before the clay is applied to thermometric purpoſes, being ſtrictly no other than preparatory proceſſes, the thermometer-pieces, whatever *clay* they may be made of (provided it is ſufficiently unvitreſcible), are to be conſidered as poſſeſſing only the fourth ſtage. But a ſingular property of the *compoſition* of clay and alum earth remains to be mentioned, *viz.* that it has really no other than this one ſtage: it ſuffers no enlargement of its bulk at ignition, or in any other period; but proceeds in one uninterrupted courſe of diminution, from the ſoft ſtate in which the pieces are formed, up to the extreme fires of our furnaces. Though the diminution, however, is uninterrupted, it is at the ſame time ſo inconſiderable at the beginning, from the heat of boiling water (at which the pieces are adjuſted) up to ignition, that the ſame point of viſible redneſs is taken for the commencement of the ſcale, in this as in the original clay, without any ſenſible error or variation in their progreſs.

I am inclined to believe, though experiments have not yet enabled me to ſpeak with certainty on this point, that the ſame cauſe which enlarges the *natural clays* on their firſt expoſure to the fire, operates alſo in this *compoſition*, but in a much lower degree; that while the natural *clays* have their whole maſs diſtended by the efforts of the air in forcing its paſſage, the *compoſition* is only reſtrained in its diminution, or prevented from diminiſhing ſo faſt as it otherwiſe would do, and as it is found to do in the ſubſequent part of its courſe, after the air has eſcaped from it.

As the compoſition of clay and alum earth is far more tenacious of water than the clay itſelf, and was found, after being dried by the heat of boiling water, to yield, by diſtillation in a retort, above three times as much aqueous fluid as the original thermometric clay did; it ſeems probable, that a part of this water, retained to the approach of ignition, and in a ſtate of chemical combination, may facilitate the paſſage of the air, ſerving as a vehicle to convey it off through interſtices not permeable to air alone, and conſequently enabling it to eſcape without doing that violence to the maſs, which the natural clays ſuſtain from the expulſion of their air after the water has been detached from it; for the experiments of Dr. PRIESTLEY have ſhewn, that veſſels even of burnt clay are permeable to air when they have imbibed water into their ſubſtance, though not at all ſo in a dry ſtate.

I have now communicated the reſult of a ſeries of experiments which have taken conſiderable time, attention, and labour to complete. Whether the importance of the object will juſtify me in troubling this illuſtrious Society with ſo minute a detail of the moſt material operations, and their reſults, is not for me to determine. If the thermometer ſhould not yet be brought to the perfection that may be wiſhed, I flatter myſelf that ſome abler hand may now take up the ſubject to more advantage; and that philoſophers and artiſts will not be leſs ſucceſsful in ſupplying what may ſtill be deficient, and in aſcertaining, by the *contraction of argillaceous matter*, the meaſurements and effects of the various degrees through the immenſe extent of luminous fire, than they have been with reſpect to the limited and narrow compaſs of low heat, which is meaſurable by the *expanſion of fluids*.

Fig. 1.

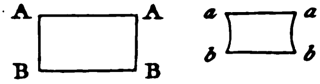


Fig. 2.

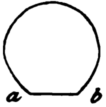
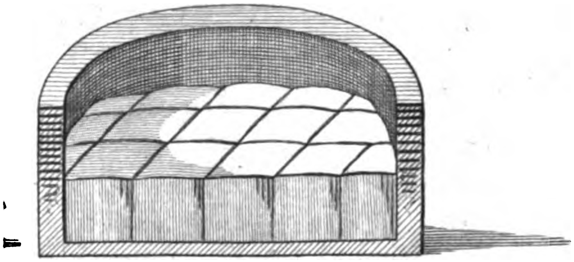


Fig. 3.



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XXIII. *The Latitude and Longitude of* York *determined from a Variety of Aſtronomical Obſervations; together with a Recommendation of the Method of determining the Longitude of Places by Obſervations of the Moon's Tranſit over the Meridian. Contained in a Letter from* Edward Pigott, *Eſq. to* Nevil Maſkelyne, *D. D. F. R. S. and Aſtronomer Royal.*

Read June 29, 1786.

SIR, Bootham, York, March 16, 1786.

THE great number and variety of obſervations I have made for determining the longitude and latitude of York will, I believe, ſettle thoſe points very accurately: I therefore wiſh to have them preſented to the Royal Society, and beg the favour of you to be at that trouble. The inſtruments I uſed were a good gridiron pendulum clock, a 2½ feet reflector, an eighteen-inch quadrant by BIRD, and a tranſit inſtrument made by SISSON.

The difference of meridians between Greenwich and York was found by the following methods.

Occultations of ſtars by the moon.

1783	App. time. h.	′	″	
Sept. 10	11	34	44½	York, immerſion of a ſtar of the ninth magnitude during the eclipſe of the moon; good.
				Paris, at L'Obſervatoire de la Marine; ditto.
	11	49	39½	Ditto, by M. Messier, who determined its R.A. 349° 22′ 17″ and ſouth declination 5° 27′ 54″.
Oct. 7	14	26	28½	York, immerſion of φ Aquarii, inſtantaneous.
	14	37	15½	Greenwich, ditto.
Dec. 30	8	1	24	York, immerſion of δ Piſcium, inſtantaneous: I find I wrote down the minute wrong, it is here corrected.
	8	2	56½	Greenwich, immerſion of ditto.

Mr. Goodricke was ſo obliging as to be at the trouble of computing theſe occultations, and ſent me the reſults as follows:

		′ ″	
By the ſtar of the ninth magnitude - - -		4 29	Difference of meridians between Greenwich and York.
By φ Aquarii - - - - -		4 23	
By δ Piſcium compared to the Greenwich obſervation	4′ 30″	4 29	
Ditto, to Mr. Wollaston's obſervation, -	4 28		
On a mean - - -		4 27	

Obſerved meridian R.A.'s of the moon's limb.

In 1783 this method of finding the difference of meridians occurred to me, and I wrote to Mr. Bayley, your late Aſſiſtant, for information, being entirely ignorant it had ever been noticed; but have ſince ſeen, Sir, that you mention it in your valuable Inſtructions for the Obſervations of the Tranſit of Venus, annexed to the Nautical Almanac of 1769. I have alſo juſt peruſed on the ſame ſubject Abbé Toaldo's ingenious pamphlet written in 1784, which you were ſo kind as to ſend me.

me. Still I find that the great exactneſs of this method is not ſuſpected; I therefore ſhall, in the latter part of this Paper, enter into ſome neceſſary detail, being convinced that, in a very ſhort time, it muſt be univerſally adopted, having every advantage over Jupiter's firſt ſatellite, and but little inferior in preciſion to occultations.

Difference of our meridians by each obſervation.

	′	″		′	″
1781, Dec. 20.	4	36	1784, July 2.	4	23
Dec. 29.	4	10	Nov. 20.	4	23
1782, June 17.	4	25	Dec. 20.	4	27
Nov. 30.	4	20	Dec. 22.	4	20
Dec. 18.	4	25	1785, Mar. 19.	4	25
1783, Nov. 3.	4	32	Aug. 16.	4	22
Dec. 6.	4	39:	Aug. 18.	4	36
Dec. 30.	4	16	Sept. 12.	4	35
1784, May 1.	4	8:	Sept. 17.	4	25
May 25.	4	11	Nov. 12.	4	34
			Nov. 14.	4	18

4′ 24″¼ on a mean.

Obſervations of Jupiter's firſt Satellite.

Dates, &c.	App. time.			
	h.	′	″	
1782, June 3.	12	36	48	York, it immerged near Jupiter.
	12	51	9	Paris, M. MECHAIN.
Immerſions.	12	51	7	Paris, M. CASSINI.
	13	57	40	Buda, Father WEISS.

Obſervations of Jupiter's firſt Satellite continued.

Dates, &c.	App. time h.	'	"	
1782, July 21	9	35	10	York.
	9	39	21	Greenwich, Dr. Maskelyne.
Emerſions.	9	48	54	Paris, M. Mechain; high wind.
	9	48	46	Paris, M. Cassini.
	10	55	15	Buda, Father Weiss; moon very near Jupiter.
1783, July 3	12	9	50	York; it immerged near Jupiter.
Immerſions.	12	14	20	Greenwich.
	12	24	8	Paris, M. Mechain.
Sept. 17	9	48	15	York.
	9	47	44	York, Mr. Goodricke; very good.
Emerſions.	9	46	39	Oxford, Mr. Hornsby.
	10	1	0	Paris, M. Mechain; very good.
1784, Aug. 4	10	10	55	York; tolerably good.
Immerſions.	10	10	57	York, Mr. Goodricke; middling.
	10	24	57	Paris, M. Mechain; air a little hazy.
Sept. 3	14	39	52	York; emerged near Jupiter.
Emerſions.	14	53	51	Paris; thinks rather too late.
Sept. 5	9	8	54	York; good.
	9	13	15	Greenwich, Dr. Maskelyne.
Emerſions.	9	22	18	Paris, M. Mechain; 6 feet reflector, magnifying 450 times.
	9	22	45	Paris; with a 3½ tripl. object glaſs achromatic.
Sept. 12	11	6	9	York; good.
	11	6	24	York, Mr. Goodricke; very good.
Emerſions.	11	10	42	Greenwich, Dr. Maskelyne.
	11	19	47	Paris, M. Mechain; as on the 5th.
	11	19	50	Paris, M. Mechain; as on the 5th.
1785, July 15	13	37	32	York; good.
Immerſions.	13	42	1	By tables corrected by the obſervations of Greenwich and Marſeilles of July 31, 1785.
July 31	11	53	18	York; good.
	11	57	32	Greenwich; air very clear.
Immerſions.	12	18	53	Marſeilles, M. Bernard.

Obſer-

Obſervations of Jupiter's firſt Satellite continued.

Dates, &c.	App. time. h.	′	″	
1785, Aug. 30	14	2	59	York; excellent; air remarkably clear.
Immerſions.	14	7	3	Greenwich; ditto.
	14	28	33	Marſeilles, M. BERNARD.
Sept. 15	12	25	2	York; good.
	12	25	4	York, Mr. GOODRICKE; good; moon-light.
Immerſions.	12	29	23	Greenwich; air clear.
	12	50	46	Marſeilles, M. BERNARD.
Nov. 18	7	58	6	York.
Emerſions.	8	2	39	Greenwich; air very clear.
	8	12	2	Paris, M. MECHAIN; a thin cloud.
Dec. 2	11	44	24	York; Jupiter rather low.
Emerſions.	11	49	13	Greenwich; ditto; air clear.

By letters from M. MECHAIN, Buda is 1 h. 6′ 33″ eaſt of Paris, and Marſeilles alſo eaſt 0 h. 12′ 7″.

I obſerved with a 2½ feet reflector, which I believe to be about 10″ of time inferior to the teleſcopes of Greenwich, Oxford, Paris, and Buda. As for Marſeilles no inſtrument is mentioned; therefore, except for that place, 10″ muſt be added to my immerſions, and the ſame ſubtracted from the emerſions; then the difference of meridians between Greenwich and York will be as follows, when each of the obſervations is compared to mine, and a mean thereof taken.

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Immerſions.		′	″	
1782,	June 3.	4	54	
1783,	July 3.	4	36	
1784,	Aug. 4.	4	36	
1785,	July 15.	4	19	
	July 31.	4	8	
	Aug. 30.	4	3	
	Sept. 15.	4	16	
		4	24½	on a mean

Emerſions.		′	″
1782,	July 21.	4	29
1782,	Sept. 17.	4	8
1784,	Sept. 3.	4	53
	Sept. 5.	4	33
	Sept. 12.	4	37
1785,	Nov. 18.	4	46
	Dec. 12.	4	59
		4	38

Therefore, by a mean of the immerſions and emerſions, York is 4′ 31″ weſt of Greenwich. Mr. Goodricke's emerſion of Sept. 17, 1783, is uſed inſtead of mine, it being undoubtedly more exact.

To enter into any detail concerning the eclipſes of Jupiter's ſatellites would be uſeleſs, as it is a matter ſo amply conſidered by every aſtronomer. I ſhall only ſay that the exactneſs expected even from thoſe of the firſt ſatellite is, in my opinion, too highly rated. Among the various objections, there is one I have often experienced, and which proceeds ſolely from the diſpoſition of the eye, that of ſeeing more diſtinctly at one time than at another. It may not be improper alſo to mention, that the obſervation I ſhould have relied on as the beſt, that of Auguſt 30, 1785, marked excellent, and air remarkably clear both at Greenwich and York, is one of thoſe which differ the moſt from the truth. This I remark without having the moſt diſtant inclination of drawing any concluſion; a ſingle inſtance can be of no weight.

Part

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Part of the eclipſe of the Moon, Sept. 10, 1783.

The two laſt columns ſhew the difference of meridians between Greenwich and York. The obſervations marked with an aſteriſk were made by Mr. Goodricke.

Spots obſerved.	York, by Mr. Goodricke and me. App. time.	Paris, by M. Mechain. App. time.	Paris, by M. Messier. App. time.	Diff. of meridians by M. Mechain.	Diff. of merid. by M. Messier.
	h. ′ ″	h. ′ ′	h. ′ ″	′ ″	′ ″
Galileus biſected –	9 45 32	9 58 33	– –	3 38	– –
Ariſtarchus covered –	9 49 13*	10 2 38	– –	4 2	– –
Copernicus touches	9 57 3*	10 11 18	10 11 9	4 52	4 48
	9 57 20	10 11 18	10 11 9	4 35	4 31
Copernicus biſected	9 58 33*	10 12 5	– –	4 9	– –
	9 58 55	10 12 5	– –	3 47	– –
Copernicus covered –	9 59 9*	10 12 57	10 12 41	4 25	4 14
Plato touches –	10 5 7	10 18 37	10 18 40	4 7	4 15
Plato covered –	10 6 18	10 19 52	10 19 28	4 11	3 52
Manilius touches –	10 11 26*	10 25 34	10 25 24	4 45	4 40
	10 11 33	10 25 34	10 25 24	4 38	4 33
Tycho touches –	10 11 57*	10 25 34	10 25 24	4 14	4 9
	10 11 57	10 25 34	10 25 24	4 14	4 9
Manilius covered –	10 12 47*	10 26 29	10 26 53	4 19	4 48
Tycho covered –	10 13 8*	10 27 19	10 27 8	4 48	4 42
	10 13 32	10 27 19	10 27 8	4 24	4 18
Menelaus biſected –	10 15 41	10 29 19	– –	4 15	– –
Prom. Acut. Cen. covered	10 25 26*	10 39 5	– –	4 16	– –
Proclus biſected –	10 29 00	10 42 28	– –	4 5	– –
Mare Criſium touches	10 30 18*	10 43 56	10 44 00	4 15	4 24
	10 30 18	10 43 56	10 44 00	4 15	4 24
Mare Criſium biſected	10 32 43*	10 46 34	10 46 10	4 28	4 9
Mare Criſium covered	10 35 38*	10 49 11	– –	4 10	– –
Grimaldus emerges –	12 23 30	– –	12 37 5	– –	4 17
Grimaldus biſected –	12 23 44	12 36 48	– –	3 41	– –
Grimaldus emerged	12 23 55*	12 37 25	– –	4 7	– –
	12 23 59:	12 37 25	– –	4 3	– –
Galileus emerges –	12 25 50*	– –	12 39 1	– –	3 53
Galileus biſected –	12 25 56*	12 39 16	– –	3 57	– –
Ariſtarchus biſected.	12 28 59	12 43 8	– –	4 46	– –

Difference of meridians on a mean 4′ 16″

M. Mechain's Obſervatory was 9′ 23″, and M. Messier's 9′ 18″ eaſt of Greenwich.

Thus I have given a comparative view of the different methods I employed in ſettling the longitude of our Obſervatory, which is in Bootham, about 400 or 500 yards N. W. of the Minſter. The occultations and meridian tranſits of the moon's limb, which make it $4' 25''\frac{1}{2}$, or $1° 6' 23''$, would have been quite ſufficient; but ſtill it is intereſting and uſeful to know how far the others err. With reſpect to the eclipſes of the moon's ſpots, I think that method is in general too much neglected; and that it might be relied on infinitely more, if certain circumſtances were mutually attended to.

1ſt, To be particular in ſpecifying the clearneſs of the ſky; for in hazy weather the reſults are very erroneous.

2dly, To chuſe ſuch ſpots that are well defined, and leave no heſitation as to the part eclipſed.

3dly, That every obſerver ſhould, as much as poſſible, uſe teleſcopes equally powerful; at leaſt let the magnifying powers be the ſame.

A principal objection may ſtill be urged, *viz.* the difficulty of diſtinguiſhing the true ſhadow from the penumbra. Was this obviated, I believe, the reſults would be more exact than from Jupiter's firſt ſatellite: undoubtedly the ſhadow appears better defined if magnified little; but I am much inclined to think, that with high magnifying powers there is greater certainty of chuſing the ſame part of the ſhadow, which perhaps is more than a ſufficient compenſation for the loſs of diſtinctneſs.

Concerning the meridian obſervations of the moon's limb.

The advantages and preciſion of this method for determining the difference of meridians is, as I have already ſaid, ſo little ſuſpected, that I flatter myſelf, the particulars I am going to mention will not be thought ſuperfluous.

The rule I adopted is this:

The increaſe of the moon's R.A. in 12 hours (or any given time) found by computation, is to 12 hours as the increaſe of the moon's R.A. between two places, found by obſervation, is to the difference of meridians.

EXAMPLE.

November 30, 1782.

h. ′ ″
13 12 57,62 meridian tranſit of the moon's ſecond limb } at Greenwich by clock.
13 13 29,08 ditto of α ♍ }

31,46 Difference of R.A.

13 14 8,05 meridian tranſit of the moon's ſecond limb } at York by clock.
13 14 30,13 ditto of α ♍ }

22,08 difference at York, } the clocks going nearly ſidereal time no correction is required.
31,46 difference at Greenwich, }

9,38 increaſe of the moon's apparent R.A. between Greenwich and York, by obſervation.

141″ in ſeconds of a degree, ditto, ditto, ditto.

The increaſe of the moon's R.A. for 12 hours by computation is 23340 ſeconds,
and 12 hours reduced into ſeconds is 43200;

therefore, according to the rule ſtated above,

$$23340'' : 43200'' :: 141'' : \text{difference of meridians} = 261'' -$$

Theſe eaſy obſervations and ſhort reduction are the whole of the buſineſs. Inſtead of computing the moon's R.A. for 12 hours, I have conſtantly taken it from the Nautical Almanacs, which give it ſufficiently exact, provided ſome attention be paid to the increaſe or decreaſe of the moon's motion.

Were the following circumſtances attended to, the reſults would undoubtedly be much more exact.

1ſt, Compare the obſervations to the ſame made in ſeveral other places.

2dly, Let ſeveral and the ſame ſtars be obſerved at theſe places.

3dly, Such ſtars as are neareſt in R.A. and declination to the moon are infinitely preferable.

4thly, Your advice to get as near as poſſible an equal number of obſervations of each limb, to take a mean of each ſet, and then a mean of both means, cannot be too ſtrongly urged. I am perfectly of your opinion, that it will conſiderably correct the error of teleſcopes and ſight.

5thly, The adjuſtment of the teleſcopes to the eye of the obſerver before the obſervation, which you alſo recommend, will appear very judicious to every aſtronomer, who muſt have frequently perceived what you mention, that the ſight is ſubject to vary.

6thly, As a principal error proceeds from the obſervation of the moon's limb, I think it may be conſiderably leſſened, if certain little round ſpots near each limb were alſo obſerved in ſettled Obſervatories; in which caſe the libration of the moon will perhaps be a conſideration.

7thly, When the difference of meridians, or of the latitudes of the places, is very conſiderable, the change of the moon's diameter becomes an equation.

Though ſuch are the requiſites to uſe this method with advantage, only one or two of them have been employed in the obſervations that I have reduced. Two thirds of theſe obſervations had not even the ſame ſtars obſerved at Greenwich and York; and yet none of the reſults, except a doubtful one,

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differ 15″ from the mean; therefore, I think, we may expect a ſtill greater exactneſs, perhaps within 10″, if the above particulars be attended to.

When the ſame ſtars are not obſerved, it is neceſſary for the obſervers at both places to compute their R.A. from tables, in order to get the apparent R.A. of the moon's limb; though this is not ſo ſatisfactory as by actual obſervation, ſtill the difference will be trifling, provided the ſtars R.A.'s are accurately ſettled. Your catalogue undoubtedly may be depended on the moſt, and thoſe ſtars preferred which have their proper motions aſcertained. A few years ago, I had the pleaſure of communicating to you the proper motion of β Virginis, which I found to be 1″,02 *per* year, increaſing in R.A.*: was this unknown, and that ſtar obſerved alone with the moon, it would occaſion, at this time, a very conſiderable error.

I am alſo of opinion, that the ſame method can be put in practice by travellers with little trouble, and a tranſit inſtrument conſtructed ſo as to fix up with facility in any place. Though I have not conſidered this ſufficiently, I ſhall, neverthele ſs, ſubjoin a few remarks that may engage others to turn their thoughts more fully to the ſubject.

It is not neceſſary, perhaps, that the inſtrument ſhould be perfectly in the meridian to a few ſeconds of time, provided ſtars, nearly in the *ſame parallel of declination* with the moon, are obſerved: nay, I am inclined to think, that if the inſtrument deviates even a quarter or half of a degree, or more, ſufficient exactneſs can be obtained, as a table might be computed, ſhewing the moon's parallax and motion for ſuch deviation, which deviation may eaſily be found by the well known method of obſerving ſtars whoſe difference of declination is conſiderable.

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* Some time previous to this communication, I had found, by the compariſon of my tranſit obſervations of α Aquilæ and β Virginis, that the latter had moved forward with a proper motion of 0″,91 of time, or of 13″,65 of R.A. from 1767 to 1783, in 16 years, or at the rate of 0″,853 a year, on ſuppoſitiou that the proper motion of α Aquilæ is 0″,57 a year forward.

As travellers very ſeldom meet with ſituations to obſerve ſtars near the pole, or find a proper object for determining the error of the line of collimation, I ſhall recommend the following idea, which, I believe, has never yet been noticed, and hope it will anſwer the purpoſe. Having computed the apparent R.A. of four, ſix, or more ſtars, which have nearly the ſame parallel of declination, obſerve half of them with the inſtrument inverted, and the other half when in its right poſition; if the difference of R.A.'s between each ſet by obſervation agree with the computation, there is no error; but if they diſagree, half that diſagreement is the error of the line of collimation. The ſame obſervations may alſo ſerve to determine whether the diſtance of the correſponding wires are equal. In caſe of neceſſity, each limb of the ſun might be obſerved in the ſame manner, though probably with leſs preciſion. By a ſingle trial I made above two years ago, the reſult was much more exact than I expected. MAYER's Catalogue of Stars will prove of great uſe to thoſe that adopt the above method.

In ſuch a number of obſervations, it is not ſurpriſing that a few ſhould be erroneous; I have rejected only three.

	′	″
A meridian tranſit of the moon's limb, Auguſt 18, 1782; δ Sagitarii was the only ſtar obſerved at York; it gives for difference of meridians, . . .	3	55
Perhaps the ſtar has a proper motion, or a miſtake of one ſecond might have been made in marking the clock.		
An immerſion of Jupiter's firſt ſatellite, June 22, 1783, which make the difference of meridians, ..	3	42

The air was hazy both at Greenwich and at York.

Laſtly, an occultation of a ſtar of the ninth magnitude, immerged behind the dark limb of the moon, during the eclipſe of Sept 10, 1783, at 11 h. 29′ 6″ apparent time. M. Messier alſo obſerved it at 11 h. 50′ 49″¼ apparent time at Paris: he determined its R.A. 349° 22′ 17″, and declination 5° 38′ 23″ ſouth. M. Goodricke, who computes very accurately, finds it gives for difference of meridians, . . . 4 44¾

I am rather ſurpriſed, that the immerſions of known ſtars of the ſixth and ſeventh magnitude behind the *dark limb* of the moon are not conſtantly obſerved in fixed Obſervatories, as they would frequently be of great uſe.

Latitude of York.

The following determinations for the latitude of York were made with a Bird's 18-inch quadrant, the teleſcope of two feet focus, with which inſtrument obſervations of the ſame ſtar ſeldom differ 10″.

Latitude of the Obſervatory.

°	′	″			
53	57	37	by 7	obſervations	of Arcturus.
53	57	41	by 2	ditto	of α Lyræ.
53	57	52	by 1	ditto	of β Arietis.
53	57	37	by 1	ditto	of β Cygni.
53	57	33	by 2	ditto	of Algol.
53	57	57	by 4	ditto	of γ Lyræ.
53	57	49	by 8	ditto	of β Draconis.
53	57	46	by 6	ditto	of μ Draconis.
53	57	56	by 2	ditto	of γ Draconis,
53	57	45+	latitude on a mean.		

The line of collimation was deduced from β, γ, and μ Draconis; half of each ſet obſerved with the face of the quadrant to the eaſt, and half with its face to the weſt. This, as well as the other methods, is very tedious, particularly when required to be often repeated, as is the caſe in travelling; I ſhall therefore propoſe the following invention, the idea of which was improved on by Mr. SMEATON, and flatter myſelf it will prove of the greateſt facility.

The error of the line of collimation includes the fixed errors of the inſtrument, and thoſe that are ſubject to change, occaſioned by the wires and glaſſes, &c. of the teleſcope moving. The error of theſe laſt may be found by making the teleſcope turn on its center, ſo that the ſun, ſtars, or terreſtrial objects may be obſerved on the horizontal wire in two manners; firſt, when the wire is in its natural poſition, and then inverted, which is performed by turning the teleſcope 180 degrees, or half round: thus, this part of the error can always be known with the greateſt eaſe; and in order to find the fixed errors, it is requiſite for *a ſingle time* to get the *whole error* of the line of collimation by one of the common methods, from which the error of the teleſcope being deducted, the fixed errors become known; and as they are unchangeable, if any alteration ſhould take place, it proceeds from the teleſcope, and may eaſily be detected as ſhewn above. Perhaps, inſtead of the whole teleſcope, it would be ſufficient only to make that part turn containing the eye-glaſs and wires.

As the following obſervations made alſo at York may be of uſe, I beg, Sir, you will annex them to my paper on the longitude and latitude of that city, which lately I had the pleaſure of ſending you.

Dates.	App. time.			
	h.	′	″	
1781, July 19	9	41	59	Emerſion of Jupiter's ſecond ſatellite; night fine.
1782, May 24	12	23	12	Immerſion of Jupiter's ſecond ſatellite; good.
July 20	11	27	40:	Emerſion of Jupiter's 2d ſat.; doubtful; air very hazy.
Nov. 30	20	57	16	Immerſion of * ♍ behind the moon; inſtantaneous.
	20	57	21½*	Ditto ditto; in another part of the town.
				Eclipſe of the moon.
1783, Mar. 18	8	27	50	Total immerſion of the moon; air very clear.
	8	27	33*	Ditto; good.
	10	9	36	Moon begins to emerge; } air hazy.
	10	10	18	Certainly emerged;
June 26	13	35	21	Immerſion of Jupiter's ſecond ſatellite; good.
	13	34	52*	Ditto; middling.
				Eclipſe of the moon; air clear.
Sept. 10	9	30	45	Appearance of penumbra.
	12	17	30	Moon not emerged, but light ſtrong.
	12	19	35	Ditto; very ſtrong.
	12	21	14	Moon begins to emerge, but uncertain.
	12	21	44	Ditto; more certain.
	12	21	56*	Ditto; ditto.
	12	22	24	Moon certainly emerged.
	12	22	24*	Ditto.
	13	21	00	End of the eclipſe, doubtful; air hazy.
	13	21	23*	Ditto.
	13	22	18*	Certainly ended, but not clear of penumbra.
	13	22	45	Ditto, ditto; air clearer.
				Several ſpots were obſerved, but are here omitted, for fear of being too voluminous.
Sept. 16	10	22	41	Emerſion of Jupiter's ſecond ſatellite; air clear; but Jupiter low.
23	9	27	18	Emerſion of Jupiter's 3d ſat.; Jupiter low; undulation.
Oct. 11	7	34	9*	Emerſion of Jupiter's ſecond ſatellite.
	7	34	21	Ditto; tolerably good.
29	5	42	53	Emerſion of Jupiter's third ſatellite.
	5	46	16	Equal in brightneſs to the ſecond ſatellite; air clear.
1784, July 27	10	7	46	Immerſion of Jupiter's third ſatellite; tolerably good, though undulation.

Dates.

Dates.	App. time.				
	h.	′	″		
1784, Aug. 26	8	54	12	Immerſion of τ ♐ behind the moon; inſtantaneous.	
Oct. 11	9	49	30	Emerſion of Jupiter's ſecond ſatellite; good, though ſlight haze.	
	9	49	26*	Ditto.	
Nov. 12	9	33	59	Emerſion of Jupiter's ſecond ſatellite.	
	9	34	1*	Ditto.	
1785, July 15	12	26	50	Immerſion of Jupiter's ſecond ſatellite; air clear.	
Aug. 18	11	44	37	Immerſion of Jupiter's third ſatellite; good;	the air a little vapouriſh.
Sept. 17	12	16	55	Immerſion of Jupiter's ſecond ſatellite; good;	
Oct. 29	6	33	26	Emerſion of Jupiter's third ſatellite;	
Nov. 15	9	24	±	I examined Jupiter's fourth ſatellite during 20′, without being certain whether it had diminiſhed in light.	
Dec. 15	5	50	48	Immerſion of 125 ♉ by the moon, exact within 3″.	

I have again marked with an aſteriſk the obſervations made by Mr. GOODRICKE, who deſired me to communicate them. This worthy young man exiſts no more; he is not only regretted by many friends, but will prove a loſs to aſtronomy, as the diſcoveries he ſo rapidly made ſufficiently evince: alſo his quickneſs in the ſtudy of mathematics was well known to ſeveral perſons eminent in that line.

Declination of the needle.

		h		°	′	
1780,	Sept. 13.	at 2½,	by a mean of 22 trials,	23	40½	Declination weſt.
1782,	Dec. 26.	at 0¾,	by a mean of 16 trials,	23	5+	
1783,	Nov. 14.	at 0¾,	by a mean of 19 trials,	23	59−	
1784,	Jan. 17.	at 0⅔,	by a mean of 13 trials,	23	54+	

Theſe obſervations were taken with all poſſible exactneſs; the needle was four inches long, and made by DOLLOND.

Sir H. ENGLEFIELD, when at Scarborough, in Auguſt and September, 1781, was ſo kind as to obſerve, at noon, the height of his barometer and thermometer. I alſo made ſimilar obſervations in the Obſervatory at York; from which, by eight compariſons, none diſagreeing above 0,018 of an inch from the mean, I find, that the quickſilver at the ſea ſtood 0,063 of an inch higher than at York. The barometers were made by RAMSDEN, and they agreed together to 0,005 part of an inch. We may later alſo expect to get the mean height of the barometer and thermometer, as there are ſeveral gentlemen that obſerve them every day, particularly Mr. WYVIL and Dr. WHITE at York, and Mr. CHOMONLEY at Branſby.

I remain, Sir, with great regard, &c.

EDW. PIGOTT.

May 26, 1786.

XXIV. *Advertiſement of the expected Return of the Comet of 1532 and 1661 in the Year 1788. By the Rev.* Nevil Maſkelyne, *D. D. F. R. S. and Aſtronomer Royal.*

Read June 29, 1786.

THE comet of 1531, 1607, and 1682, having returned in the year 1759, according to Dr. HALLEY's prediction in his *Synopſis Aſtronomiæ Cometicæ*, firſt publiſhed in the Philoſophical Tranſactions in 1705, and re-publiſhed with his Aſtronomical Tables in 1749, there is no reaſon to doubt that all the other comets will return after their proper periods, according to the remark of the ſame author.

In the firſt edition of the *Synopſis* he ſuppoſed the comets of 1532 and 1661, from the ſimilarity of the elements of their orbits, to be one and the ſame; but in the ſecond edition he has ſeemed to leſſen the weight of his firſt conjecture by not repeating it. Probably he thought it beſt to eſtabliſh this new point in aſtronomy, the doctrine of the revolution of comets in elliptic orbits, as all philoſophical matters in the beginning ſhould be, on the moſt certain grounds; and feared that the vague obſervations of the comet, made by APIAN in 1532, might rather detract from, than add to, the evidence ariſing from more certain *data*. Aſtronomers, however, have generally acquieſced in his firſt conjecture of the comets of 1532 and 1661 being one and the ſame, and to expect its return to its perihelium accordingly in 1789.

The interval between the paſſages of the comet by the perihelium in 1532 and 1661 is 128 years, 89 days, 1 hour, 29 minutes (32 of the years being biſſextile), which added to the time of the perihelium in 1661, together with 11 days to reduce it from the Julian to the Gregorian ſtile, which we now uſe, brings out the expected time of the next perihelium to be April 27th, 1 h. 10′ in the year 1789.

The periodic times of the comet, which appeared in 1531, 1607, and 1682, having been of 76 and 75 years alternately, Dr. HALLEY ſuppoſed, that the ſubſequent period would be of 76 years, and that it would return in the year 1758; but, upon conſidering its near approach to Jupiter, in its deſcent towards the ſun in the ſummer of 1681, he found, that the action of Jupiter upon the comet was, for ſeveral months together, equal to one-fiftieth part of the ſun upon it, tending to increaſe the inclination of the orbit to the plane of the ecliptic, and lengthen the periodic time. Accordingly, the inclination of the orbit was found by the obſervations made in the following year 1682 to be 22′ greater than in the year 1607. The effect of the augmentation of the periodic time could not be ſeen till the next return, which he ſuppoſed would be protracted by Jupiter's action to the latter end of the year 1758, or the beginning of 1759. M. CLAIRAUT, previous to its return, took the pains to calculate the actions both of Jupiter and Saturn on it during the whole periods from 1607 to 1682, and from 1682 to 1759, and thence predicted its return to its perihelium by the middle of April; it came about the middle of March, only a month ſooner, which was a ſufficient approximation to the truth in ſo delicate a matter, and did honour to this great mathematician, and his laborious calculations.

The comet in queſtion is alſo, from the poſition of its orbit, liable to be much diſturbed both by Jupiter and Saturn, particularly in its aſcent from the ſun after paſſing its perihelium, if they ſhould happen to be near it, when it appoaches to or croſſes their orbits; becauſe it is very near the plane of them at that time. When it paſſed the orbit of Jupiter in the beginning of February 1682, O. S. it was 50° *in conſequentia* of that planet; and when it paſſed the orbit of Saturn in the beginning of October 1663, it was 17° *in conſequentia* of it. Hence its motion would be accelerated while it was approaching towards the orbit of either planet by its ſeparate action, and retarded when it had paſſed its orbit; but, as it would be ſubjected to the effect of retardation through a greater part of its orbit than to that of acceleration, the former would exceed the latter, and conſequently the periodic time would be ſhortened; but probably not much, on account of the conſiderable diſtance of the comet from the planets when it paſſed by them; and therefore we may ſtill expect it to return to its perihelium in the beginning of the year 1789, or the latter end of the year 1788, and certainly ſome time before the 27th of April 1789. But of this we ſhall be better informed after the end of this year, from the anſwers to the prize queſtion propoſed by the Royal Academy of Sciences at Paris, to compute the diſturbances of the comet of 1532 and 1661, and thence to predict its return *.

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* Since this was written, I received the unwelcome news, in a letter from M. MECHAIN, of the Royal Academy of Sciences at Paris, that the Academy has not received ſatisfactory anſwers concerning the diſturbances of the comet between 1532 and 1661, and 1661 and the approaching return, and that the prize is referred to be adjudged of at Eaſter 1788, and that it will be 6000 livres. N. M.

If it ſhould come to its perihelium on the 1ſt of January 1789, it might probably be viſible, with a good achromatic teleſcope, in its deſcent to the ſun, the middle of September 1788, and ſooner or later, according as its perihelium ſhould be ſooner or later. It will approach us from the ſouthern parts of its orbit, and therefore will firſt appear with conſiderable ſouth latitude and ſouth declination; ſo that perſons reſiding nearer the equator than we do, or in ſouth latitude, will have an opportunity of diſcovering it before us. It is to be wiſhed that it may be firſt ſeen by ſome aſtronomer in ſuch a ſituation, and furniſhed with proper inſtruments for ſettling its place in the heavens, the earlieſt good obſervations being moſt valuable for determining its elliptic orbit, and proving its identity with the comets of 1532 and 1661. The Cape of Good Hope would be an excellent ſituation for this purpoſe.

In order to aſſiſt aſtronomers in looking out for this comet, I have here given its heliocentric and geocentric longitudes and latitudes and correſpondent diſtances from the ſun and earth, on ſuppoſition that it ſhall come to its perihelium on January 1, 1789. But if that ſhould happen ſooner or later, the heliocentric longitudes and latitudes and diſtances from the ſun will ſtand good if applied to days as much earlier or later, as the time of the perihelium may happen ſooner or later; and the geocentric longitudes and latitudes and diſtances from the earth muſt be re-computed accordingly. The calculations are made for a parabolic orbit from the elements determined by Dr. HALLEY from HEVELIUS's obſervations in 1661, only allowing for the preceſſion of the equinoxes. The elements made uſe of were as follows:

Time of perihelium January 1, 1789, at noon.

Perihelium diſtance 0,44851.

Place of aſcending node 2^s 24° 18′.

Inclination of orbit to the ecliptic 32° 36′.

Perihelium forwarder in orbit than the aſcending node 33° 28′.

It motion is direct.

Computed places of the comet, on ſuppoſition that it ſhall return to its perihelium January 1, 1789, at noon.

Times.	Diſt. from ☉.	Diſt. from the earth.	Heliocentric longitude.			Heliocentric latitude.		Geocentric longitude.			Geocentric latitude.		Product of diſtances from ☉ and earth.
1788			S.	D.	M	D.	M.	S.	D.	M.	D.	M.	
Apr. 23, 7	4, 0	4,52	11	3	54	30	56 S	11	16	30	27	5 S	18,07
June 4, 1	3, 5	3,54	11	7	6	31	25	11	26	31	31	4	12,38
July 14, 5	3,	2,57	11	11	16	31	55	0	3	21	38	11	7,70
Aug. 2,46	2,75	2,15	11	13	47	32	10	0	4	8	42	59	5,90
— 20,43	2, 5	1,79	11	16	39	32	22	0	2	0	48	16	4,48
Sept. 7, [illegible]	2,25	1,51	11	20	9	32	32	11	25	6	53	28	3,39
— 24, 0	2,	1,29	11	24	16	32	36	11	13	12	56	45	2,58
Oct. 10,26	1,75	1,13	11	29	24	32	30	10	28	22	56	36	1,75
— 26,64	1,50	1,01	0	5	51	32	4	10	15	50	52	6	1,51
Nov. 9,34	1,25	0,88	0	14	19	31	0	10	8	36	46	47	1,10
— 23,39	1, 0	0,76	0	26	4	28	32	10	4	10	39	0	0,76
Dec. 7,21	0,75	0,62	1	13	58	22	29	9	29	18	27	45	0,46
— 23,3[illegible]	0,50	0,50	2	20	58	2	8	9	14	31	2	7 S	0,25
— 24,35	0,49	0,51	2	24	18	0	0	9	12	58	0	0	0,25
1789													
Jan. 1, 0	0,45	0,59	3	23	25	17	17 N	9	2	50	13	8 N	0,26

The laſt obſervation made by HEVELIUS on the comet in 1661 was when its diſtance from the earth was 0,986, and from the ſun 1,37, with what he calls a very long and good teleſcope; at which time it appeared faint and ſmall with it, though ſtill ſufficiently viſible. Let us ſuppoſe this to have been a teleſcope of 9-feet focal length, with an aperture of 1,65 inch; then, becauſe the diameter of the aperture of a teleſcope ſufficient to render the comet equally viſible ſhould be as the product of its diſtances from the ſun and earth, and the product of the numbers above-mentioned 0,986 and 1,37 is 1,35, we ſhall have the following analogy to find the aperture of a refracting teleſcope ſufficient to ſhew the comet as it appeared to Hevelius. As 1,35 : 1,65 inch :: 9 : 11 inches, ſo is the product of diſtances from the ſun and earth to the diameter of the aperture required in inches.

XXV. *A new Method of finding Fluents by Continuation. By the Rev.* Samuel Vince, *A. M. F. R. S.*

Read July 6, 1786.

ART. I. Put $\dot{F} = \frac{x^m\dot{x}}{a^n+x^n} = x^{m-n}\dot{x} - a^n x^{m-2n}\dot{x} + a^{2n}x^{m-3n}\dot{x} -$ &c. $\pm \frac{a^{vn}x^{m-vn}\dot{x}}{a^n+x^n}$, then $F = \frac{x^{m-n+1}}{m-n+1} - \frac{a^n x^{m-2n+1}}{m-2n+1} + \frac{a^{2n}x^{m-3n+1}}{m-3n+1} -$ &c. $\pm W$, where W repreſents the fluent of the laſt term. Now $\frac{x^m\dot{x}}{\overline{a^n+x^n}|^r} = \frac{x^m\dot{x}\times\overline{a^n+x^n}|^{1-r}}{a^n+x^n}$; hence $\int\frac{x^m\dot{x}}{\overline{a^n+x^n}|^r} = \int\frac{x^m\dot{x}\times\overline{a^n+x^n}|^{1-r}}{a^n+x^n} = F \times \overline{a^n+x^n}|^{1-r} - \int F\times\frac{\overline{1-r}\,.\,nx^{n-1}\dot{x}}{\overline{a^n+x^n}|^r} =$ (by ſubſtituting for F its value in the latter quantity) $F\times\overline{a^n+x^n}|^{1-r} - \frac{\overline{1-r}\,.\,n}{m-n+1}\times\int\frac{x^m\dot{x}}{\overline{a^n+x^n}|^r} + \frac{\overline{1-r}\,.\,na^n}{m-2n+1}\times\int\frac{x^{m-n}\dot{x}}{\overline{a^n+x^n}|^r} - \frac{\overline{1-r}\,.\,na^{2n}}{m-3n+1}\times\int\frac{x^{m-2n}\dot{x}}{\overline{a^n+x^n}|^r} +$ &c. $\pm \int W\times\frac{\overline{1-r}\,.\,nx^{n-1}\dot{x}}{\overline{a^n+x^n}|^r}$; tranſpoſe $-\frac{\overline{1-r}\,.\,n}{m-n+1}\times\int\frac{x^m\dot{x}}{\overline{a^n+x^n}|^r}$ and divide both ſides of the equation by $\frac{m-rn+1}{m-n+1}$ and we have $\int\frac{x^m\dot{x}}{\overline{a^n+x^n}|^r} = \frac{m-n+1}{m-rn+1}\times F\times\overline{a^n+x^n}|^{1-r} + \frac{m-n+1}{m-rn+1}\times\frac{\overline{1-r}\,.\,na^n}{m-2n+1}\times\int\frac{x^{m-n}\dot{x}}{\overline{a^n+x^n}|^r} - \frac{m-n+1}{m-rn+1}\times\frac{\overline{1-r}\,.\,na^{2n}}{m-3n+1}\times\int\frac{x^{m-2n}\dot{x}}{\overline{a^n+x^n}|^r} +$ &c. $\pm\frac{m-n+1}{m-rn+1}\times$

$\int W \times \frac{\overline{1-r}\,.\,nx^{n-1}\dot{x}}{\overline{a^n+x^n}|^r}$; now the fluent of the laſt term is $\mp\frac{m-n+1}{m-rn+1} \times W \times \overline{a^n+x^n}|^{1-r} \pm \frac{m-n+1}{m-rn+1} \times \int \frac{a^{vn}x^{m-vn}\dot{x}}{\overline{a^n+x^n}|^r}$; hence by ſubſtituting this quantity for the laſt term, it is manifeſt, that the firſt part $\mp\frac{m-n+1}{m-rn+1} \times W \times \overline{a^n+x^n}|^{1-r}$ will be deſtroyed by the laſt term of $\frac{m-n+1}{m-rn+1} \times F \times \overline{a^n+x^n}|^{1-r}$, when we ſubſtitute for F its value; hence if we put $M = \frac{x^{m-n+1}}{m-n+1} - \frac{a^n x^{m-2n-1}}{m-2n+1} + \frac{a^{2n}x^{m-3n+1}}{m-3n+1}$ – &c. omitting the laſt term ± W, we have $\int \frac{x^m\dot{x}}{\overline{x^n+x^n}|^r} = \frac{m-n+1}{m-rn+1} \times M \times \overline{a^n+x^n}|^{1-r} + \frac{m-n+1}{m-rn+1} \times \frac{\overline{1-r}\,.\,na^n}{m-2n+1} \times \int \frac{x^{m-n}\dot{x}}{\overline{a^n+x^n}|^r} - \frac{m-n+1}{m-rn+1} \times \frac{\overline{1-r}\,.\,na^{2n}}{m-3n+1} \times \int \frac{x^{m-2n}\dot{x}}{\overline{a^n+x^n}|^r} + \&c. \pm \frac{m-n+1}{m-rn+1} \times a^{vn} \times \int \frac{x^{m-vn}\dot{x}}{\overline{a^n+x^n}|^r}$; hence, if the fluent of the laſt term be given, we have the general law of continuation by which we may find the fluent of $\frac{x^m\dot{x}}{\overline{a^n+x^n}|^r}$. If the fluxion be $\frac{x^m\dot{x}}{\overline{a^n+x^n}|^r}$ all the terms after the firſt will be negative, and the laſt always poſitive.

Ex. 1. Given the fluent of $\frac{\dot{x}}{\sqrt{1+x^2}}$ to find the fluent of $\frac{x^{2s}\dot{x}}{\sqrt{1+x^2}}$.

Here $n = 2$, $a = 1$, $m = 2s$, $r = \frac{1}{2}$, $M = \frac{x^{2n-1}}{2n-1} - \frac{x^{2n-3}}{2n-3}$ + &c. to $\pm x$,

$\pm x$, and the fluent of $\frac{\dot{x}}{\sqrt{1+x^2}}$ is the hyp. log. $x+\sqrt{1+x^2}$ which call Q; hence $\int\frac{x^{2s}\dot{x}}{\sqrt{1+x^2}}=\frac{2s-1}{2s}\times M\times\sqrt{1+x^2}+\frac{2s-1}{2s\,.\,\overline{2s-3}}\times\int\frac{x^{2s-2}\dot{x}}{\sqrt{1+x^2}}-\frac{2s-1}{2s\,.\,\overline{2s-5}}\times\int\frac{x^{2s-4}\dot{x}}{\sqrt{1+x^2}}+\&c.\pm\frac{2s-1}{2s}\times Q.$

If $s=1$, $\int\frac{x^2\dot{x}}{\sqrt{1+x^2}}=\frac{1}{2}\sqrt{1+x^2}\times x-\frac{1}{2}Q=\alpha.$

$s=2$, $\int\frac{x^4\dot{x}}{\sqrt{1+x^2}}=\frac{3}{4}\sqrt{1+x^2}\times\overline{\frac{x^3}{3}-x}+\frac{3}{4}\alpha+\frac{3}{4}Q=\beta.$

$s=3$, $\int\frac{x^6\dot{x}}{\sqrt{1+x^2}}=\frac{5}{6}\sqrt{1+x^2}\times\overline{\frac{x^5}{5}-\frac{x^3}{3}+x}+\frac{5}{6\,.\,3}\beta-\frac{5}{6}\alpha-\frac{5}{6}Q=\gamma.$

$s=4$, $\int\frac{x^8\dot{x}}{\sqrt{1+x^2}}=\frac{7}{8}\sqrt{1+x^2}\times\overline{\frac{x^7}{7}-\frac{x^5}{5}+\frac{x^3}{3}-x}+\frac{7}{8\,.\,5}\gamma-\frac{7}{8\,.\,8}\beta+\frac{7}{8}\alpha+\frac{7}{8}Q.$

&c. &c. &c.

Ex. 2. To find the fluent of $x^{\frac{s}{2}}\dot{x}\sqrt{2+x}$, given the fluent of $x^{-\frac{1}{2}}\dot{x}\sqrt{2+x}$, and s an odd number.

Here $a=2$, $n=1$, $r=-\frac{1}{2}$, $\frac{s}{2}=m$, $m-vn=-\frac{1}{2}$, or $\frac{s}{2}-v=-\frac{1}{2}$; $\therefore v=\frac{s+1}{2}$, $M=\frac{2x^{\frac{s}{2}}}{s}-\frac{4x^{\frac{s}{2}-1}}{s-2}+\frac{8x^{\frac{s}{2}-2}}{s-4}-$ &c. and the fluent (Q) of $x^{-\frac{1}{2}}\dot{x}\sqrt{2+x}$ is $\pi+\sqrt{2x+x^2}$, where $\pi=$ hyp. log. $1+x+\sqrt{2x+x^2}$; hence $\int x^{\frac{s}{2}}\dot{x}\sqrt{2+x}=\frac{s}{s+3}\times\overline{2+x}|^{\frac{3}{2}}\times M+\frac{s}{s+3}\times\frac{b}{s-2}\times\int x^{\frac{s}{2}-1}\dot{x}\sqrt{2+x}-\frac{s}{s+3}\times\frac{12}{s-4}\times\int x^{\frac{s}{2}-2}\dot{x}\sqrt{2+x}+\frac{s}{s+3}\times\frac{24}{s-6}\times\int x^{\frac{s}{2}-3}\dot{x}\sqrt{2+a}$ &c $\pm\frac{s}{s+3}\times 2^{\frac{s+1}{2}}\times\int x^{-\frac{1}{2}}\dot{x}\sqrt{2+x}.$

If $s=1, \int x^{\frac{1}{2}}\dot{x}\sqrt{2+x} = \frac{1}{4}\times\overline{2+x}|^{\frac{3}{2}}\times 2x^{\frac{1}{2}} - \frac{1}{2}Q = \alpha.$

$s=3, \int x^{\frac{3}{2}}\dot{x}\sqrt{2+x} = \frac{1}{2}\times\overline{2+x}|^{\frac{3}{2}}\times\overline{\frac{2x^{\frac{3}{2}}}{3} - 4x^{\frac{1}{2}}} + 3\alpha + 2Q = \beta.$

$s=5, \int x^{\frac{5}{2}}\dot{x}\sqrt{2+x} = \frac{5}{8}\times\overline{2+x}|^{\frac{3}{2}}\times\overline{\frac{2x^{\frac{5}{2}}}{5} - \frac{4x^{\frac{3}{2}}}{3} + 8x^{\frac{1}{2}}} + \frac{5}{4}\beta -$
$\frac{15}{2}\alpha - 5Q = \gamma.$

$s=7, \int x^{\frac{7}{2}}\dot{x}\sqrt{2+x} = \frac{7}{10}\times\overline{2+x}|^{\frac{3}{2}}\times\overline{\frac{2x^{\frac{7}{2}}}{7} - \frac{4x^{\frac{5}{2}}}{5} + \frac{8x^{\frac{3}{2}}}{3} - 16x^{\frac{1}{2}}} +$
$\frac{21}{25}\gamma - \frac{14}{5}\beta + \frac{84}{5}\alpha + \frac{56}{5}Q.$

&c. &c. &c.

II. Let $\dot{F}\,\frac{x^n\dot{x}}{a+bx^m+x^{2m}} = x^{n-2m}\dot{x} - Px^{n-3m}\dot{x} + Qx^{n-4m}\dot{x} - \&c. \pm \frac{Vx^{n-sm}\dot{x}}{a+bx^m+x^{2m}} \pm \frac{Wx^{\overline{n-s+1}\cdot m}\dot{x}}{a+bx^m+x^{2m}}$, then $F = \frac{x^{n-2m+1}}{n-2m+1} - \frac{Px^{n-3m-1}}{n-3m+1} + \frac{Qx^{n-4m+1}}{n-4m+1} - \&c. \pm T \pm U$, where T and U are put for the fluents of the two laſt terms, and P, Q, &c. for the co-efficients ariſing from the diviſion. Now, $\int\frac{x^n\dot{x}}{\sqrt{a+bx^m+x^{2m}}} = \int\frac{x^n\dot{x}\times\overline{a+bx^m+x^{2m}}|^{1-r}}{a+bx^m+x^{2m}} =$ $F\times\overline{a+bx^m+x^{2m}}|^{1-r} - \int F + \frac{\overline{1-r}\cdot mbx^{m-1}\dot{x} + \overline{1-r}\cdot 2mx^{2m-1}\dot{x}}{\overline{a+bx^m+x^{2m}}|^{r}} =$ (by ſubſtituting for F its value in the latter quantity, and putting A, B, C, &c for the co-efficients which ariſe in conſequence thereof) $F\times\overline{a+bx^m+x^{2m}}|^{1-r} - A\times\int\frac{x^n\dot{x}}{\overline{a+bx^m+x^{2m}}|^{r}} + B\times\int\frac{x^{n-m}\dot{x}}{\overline{a+bx^m+x^{2m}}|^{r}} - C\times\int\frac{x^{n-2m}\dot{x}}{\overline{a+bx^m+x^{2m}}|^{r}} + \&c.$

$$\mp\int T\times\frac{\overline{1-r}\,.\,mbx^{m-1}\dot{x}+\overline{1-r}\,.\,2mx^{2m-1}\dot{x}}{\overline{a+bx^{m}+x^{2m}}\big|^{r}}$$

$\mp\int U\times\frac{\overline{1-r}\,.\,mbx^{m-1}\dot{x}+\overline{1-r}\,.\,2mx^{2m-1}\dot{x}}{\overline{a+bx^{m}+x^{2m}}\big|^{r}}$; hence by tranſpoſition and diviſion we have $\int\frac{x^{n}\dot{x}}{\overline{a+bx^{m}+x^{2m}}\big|^{r}}=\frac{1}{1+A}\times F\times\overline{a+bx^{m}+x^{2m}}\big|^{1-r}$ $+\frac{B}{1+A}\times\int\frac{x^{n-m}\dot{x}}{\overline{a+bx^{m}+x^{2m}}\big|^{r}}-\frac{C}{1+A}\times\int\frac{x^{n-2m}\dot{x}}{\overline{a+bx^{m}+x^{2m}}\big|^{r}}+$ &c.

$$\mp\int\frac{T}{1+A}\times\frac{\overline{1-r}\,.\,mbx^{m-1}\dot{x}+\overline{1-r}\,.\,2mx^{2m-1}\dot{x}}{\overline{a+bx^{m}+x^{2m}}\big|^{r}}$$

$\mp\int\frac{U}{1+A}\times\frac{\overline{1-r}\,.\,mbx^{m-1}\dot{x}+\overline{1-r}\,.\,2mx^{2m-1}\dot{x}}{\overline{a+bx^{m}+x^{2m}}\big|^{r}}$. Now the fluents of theſe two laſt terms are $\mp\frac{T}{1+A}\times\overline{a+bx^{m}+x^{2m}}\big|^{1-r}\pm\frac{V}{1+A}$ $\int\frac{x^{n-sm}\dot{x}}{\overline{a+bx^{m}+x^{2m}}\big|^{r}}$ and $\mp\frac{U}{1+A}\times\overline{a+bx^{m}+x^{2m}}\big|^{1-r}\pm\frac{W}{1+A}\int\frac{x^{n-\overline{s+1}\,.\,m}\dot{x}}{\overline{a+bx^{m}+x^{2m}}\big|^{r}}$ reſpectively; hence, by ſubſtituting theſe for the laſt term, it is manifeſt that $\mp\frac{T}{1+A}\times\overline{a+bx^{m}+x^{2m}}\big|^{1-r}$ and $\mp\frac{U}{1+A}$ $\times\overline{a+bx^{m}+x^{2m}}\big|^{1-r}$, will be deſtroyed by the two laſt terms of $\frac{1}{1+A}\times F\times\overline{a+bx^{m}+x^{2m}}\big|^{1-r}$ when we ſubſtitute for F its value; hence, if we put $M=\frac{x^{n-2m-1}}{n-2m+1}-\frac{Px^{n-3m+1}}{n-3m+1}+\frac{Qx^{n-4m+1}}{n-4m+1}-$ &c. omitting the two laſt terms $\pm$ T and $\pm$ U, we ſhall have $\int\frac{x^{n}\dot{x}}{\overline{a+bx^{m}+x^{2m}}\big|^{r}}=\frac{1}{1+A}\times M\times\overline{a+bx^{m}+x^{2m}}\big|^{1-r}+\frac{B}{1+A}\times$ $\int\frac{x^{n-m}\dot{x}}{\overline{a+bx^{m}+x^{2m}}\big|^{r}}-\frac{C}{1+A}\times\int\frac{x^{n-2m}\dot{x}}{\overline{a+bx^{m}+x^{2m}}\big|^{r}}+$ &c. $\pm\frac{V}{1+A}\times$

$\int \frac{x^{n-sm}\dot{x}}{\overline{a+bx^m+x^{2m}}|^{r}} \pm \frac{W}{1+A} \times \frac{x^{n-\overline{s+1}\cdot m}\dot{x}}{\overline{a+bx^m+x^{2m}}|^{r}}$; hence if the two laſt fluents be given, we have the general law of continuation up to $\frac{x^n\dot{x}}{\overline{a+bx^m+x^{2m}}|^{r}}$ in the ſame manner as before.

III. In general, if we proceed as in the two laſt articles, we ſhall find $\int \frac{x^n\dot{x}}{\overline{a+bx^m+\&c.\ x^{tm}}|^{r}} = \frac{M}{P} \times \overline{a+bx^m+\&c.\ x^{tm}}|^{1-r} + \frac{A}{P} \times \int \frac{x^{n-m}\dot{x}}{\overline{a+bx^m+\&c.\ x^{tm}}|^{r}} + \frac{B}{P} \times \int \frac{x^{n-2m}\dot{x}}{\overline{a+bx^m+\&c.\ x^{tm}}|^{r}} + \&c. \pm \frac{T}{P} \times \int \frac{x^{n-sm}\dot{x}}{\overline{a+bx^m+\&c.\ x^{tm}}|^{r}} \pm \frac{V}{P} \times \int \frac{x^{n-\overline{s+1}\cdot m}\dot{x}}{\overline{a+bx^m+\&c.\ x^{tm}}|^{r}} \pm \&c.$ where the number of theſe laſt terms is t, and $M = \frac{x^{n-tm+1}}{n-tm+1} - \frac{Qx^{n-\overline{t+1}\cdot m+1}}{n-\overline{t+1}\cdot m+1} + \&c.$ omitting, as before, the terms at the end ariſing from the remainders. Hence if the laſt t fluents be given, we can by continuation find the required fluent.

Becauſe the diviſion of $\frac{x^n\dot{x}}{a+bx^m+\&c.\ x^{tm}}$ may be expreſſed by an *aſcending* ſeries $x^n\dot{x} - Qx^{n+m}\dot{x} + Rx^{n+2m}\dot{x} - \&c.$ it is manifeſt, that by the ſame method we may continue the fluents downwards as well as upwards.

IV. Let $\dot{F} = \frac{x^n\dot{x}}{1-x} = -x^{n-1}\dot{x} - x^{n-2}\dot{x} - x^{n-3}\dot{x} - \&c. - x^{n-r}\dot{x} + \frac{x^{n-r}\dot{x}}{1-x}$, then $F = -\frac{x^n}{n} - \frac{x^{n-1}}{n-1} - \frac{x^{n-2}}{n-2} - \&c. - \frac{x^{n-r+1}}{n-r+1} + W$, where W is the fluent of the laſt term. Now $\frac{x^n\dot{x}}{\sqrt{1-x^2}} = \frac{x^n\dot{x}}{1-x} \times \sqrt{\frac{1-x}{1+x}}$, hence $\int\frac{x^n\dot{x}}{\sqrt{1-x^2}}=\int\frac{x^n\dot{x}}{1-x}\times\sqrt{\frac{1-x}{1+x}}=\mathrm{F}\times\sqrt{\frac{1-x}{1+x}}+\int\mathrm{F}\times\frac{\dot{x}}{\sqrt{1-x^2}\times\overline{1+x}}=\mathrm{F}\times\sqrt{\frac{1-x}{1+x}}-\int\frac{x^n\dot{x}}{n\sqrt{1-x^2}\times\overline{1+x}}-\int\frac{x^{n-1}\dot{x}}{n-1\,.\sqrt{1-x^2}\times\overline{1+x}}-\int\frac{x^{n-2}\dot{x}}{n-2\,.\sqrt{1-x^2}\times\overline{1+x}}-\&c.-\int\frac{x^{n-r+1}\dot{x}}{n-r+1\,.\sqrt{1-x^2}\times\overline{1+x}}+\int\mathrm{W}\times\frac{\dot{x}}{\sqrt{1-x^2}\times\overline{1+x}}$. But

$$\frac{x^n\dot{x}}{n\sqrt{1-x^2}\times\overline{1+x}}=\frac{x^{n-1}\dot{x}}{n\sqrt{1-x^2}}-\frac{x^{n-2}\dot{x}}{n\sqrt{1-x^2}}+\frac{x^{n-3}\dot{x}}{n\sqrt{1-x^2}}-\&c.\mp\frac{x^{n-r}\dot{x}}{n\sqrt{1-x^2}}\pm\frac{x^{n-r}\dot{x}}{n\sqrt{1-x^2}\times 1+}$$

$$\frac{x^{n-1}\dot{x}}{n-1\,.\sqrt{1-x^2}\times\overline{1+x}}=\frac{x^{n-2}\dot{x}}{n-1\,.\sqrt{1-x^2}}-\frac{x^{n-3}\dot{x}}{n-1\,.\sqrt{1-x^2}}+\&c.\pm\frac{x^{n-r}\dot{x}}{n-1\,.\sqrt{1-x^2}}\mp\frac{x^{n-r}\dot{x}}{n-1\,.\sqrt{1-x^2}\times 1}$$

$$\frac{x^{n-2}\dot{x}}{n-2\,.\sqrt{1-x^2}\times\overline{1+x}}=\frac{x^{n-3}\dot{x}}{n-2\,.\sqrt{1-x^2}}-\&c.\mp\frac{x^{n-r}\dot{x}}{n-2\,.\sqrt{1-x^2}}\pm\frac{x^{n-r}\dot{x}}{n-2\,.\sqrt{1-x^2}\times 1}$$

&c. &c. &c.

$$\frac{x^{n-r+1}\dot{x}}{n-r+1\,.\sqrt{1-x^2}\times\overline{1+x}}=\frac{x^{n-r}\dot{x}}{n-r+1\,.\sqrt{1+x^2}}-\frac{x^{n-r}\dot{x}}{n-r+1\,.\sqrt{1-x^2}\times 1+}$$

Hence $\int\frac{x^n\dot{x}}{\sqrt{1-x^2}}=\mathrm{F}\times\sqrt{\frac{1-x}{1+x}}-\frac{1}{n}\times\int\frac{x^{n-1}\dot{x}}{\sqrt{1-x^2}}+\overline{\frac{1}{n}-\frac{1}{n-1}}\times\int\frac{x^{n-2}\dot{x}}{\sqrt{1-x^2}}-\overline{\frac{1}{n}-\frac{1}{n-1}+\frac{1}{n-2}}\times\int\frac{x^{n-3}\dot{x}}{\sqrt{1-x^2}}+\&c.\overline{\pm\frac{1}{n}\mp\frac{1}{n-1}\pm\&c.-\frac{1}{n-r+1}}\times\int\frac{x^{n-r}\dot{x}}{\sqrt{1-x^2}}\overline{\mp\frac{1}{n}\pm\frac{1}{n-1}\mp\&c.+\frac{1}{n-r+1}}\times\int\frac{x^{n-r}\dot{x}}{\sqrt{1-x^2}\times\overline{1+x}}+\int\mathrm{W}\times\frac{\dot{x}}{\sqrt{1-x^2}\times\overline{1+x}}$. Now $\int\mathrm{W}\times\frac{\dot{x}}{\sqrt{1-x^2}\times\overline{1+x}}=-\mathrm{W}\times\overline{\frac{1-x}{1+x}}\Big|^{\frac{1}{2}}+\int\frac{x^{n-r}\dot{x}}{\sqrt{1-x^2}}$; alſo $\int\frac{x^{n-r}\dot{x}}{\sqrt{1-x^2}\times\overline{1+x}}=-x^{n-r}\times\sqrt{\frac{1-x}{1+x}}+\overline{n-r}\times\int\frac{x^{n-r-1}\dot{x}}{\sqrt{1-x^2}}-\overline{n-r}\times\int\frac{x^{n-r}\dot{x}}{\sqrt{1-x^2}}$; hence, by ſubſtituting theſe quantities in the two laſt terms, it is manifeſt that $-\mathrm{W}\times\overline{\frac{1-x}{1-x}}\Big|^{\frac{1}{2}}$ will be deſtroyed by the laſt term of

F ×

$F\times\overline{\frac{1-x}{1+x}}\Big|^{\frac{1}{2}}$ when we ſubſtitute for F its value; therefore, if we put $M=-\frac{x^n}{n}-\frac{x^{n-1}}{n-1}-\frac{x^{n-2}}{n-2}-\&c.-\frac{x^{n-r+1}}{n-r+1}$, we ſhall have

$$\int\frac{x^n\dot{x}}{\sqrt{1+2}}=M\overline{\pm\frac{1}{n}\mp\frac{1}{n-1}\pm\&c.-\frac{1}{n-r+1}}\times\overline{\frac{1-x}{1+x}}\Big|^{\frac{1}{2}}-\frac{1}{n}\times\int\frac{x^{n-1}\dot{x}}{\sqrt{1-x^2}}$$

$$+\overline{\frac{1}{n}-\frac{1}{n-1}}\times\int\frac{x^{n-2}\dot{x}}{\sqrt{1-x^2}}-\overline{\frac{1}{n}-\frac{1}{n-1}+\frac{1}{n-2}}\times\int\frac{x^{n-3}\dot{x}}{\sqrt{1-x^2}}+\&c.$$

$$\overline{\pm\frac{1}{n}\mp\frac{1}{n-1}\pm\&c.-\frac{1}{n-r+1}}\times\overline{n-r+1}+1\times\int\frac{x^{n-r}\dot{x}}{\sqrt{1-x^2}}\mp$$

$$\overline{\frac{1}{n}\pm\frac{1}{n-1}\mp\&c.+\frac{1}{n-r+1}}\times\overline{n-r}\times\int\frac{x^{n-r-1}\dot{x}}{\sqrt{1-x^2}}.$$

Hence, if the two laſt fluents be given, we have the general law of continuation up to the fluent of $\frac{x^n\dot{x}}{\sqrt{1-x^2}}$, where the index of x without the vinculum increaſes by unity each time. And in the ſame manner we may (by increaſing the index of x without by m) find the fluent of $\frac{x^n\dot{x}}{\sqrt{a^{2m}-x^{2m}}}$ if we have given the fluents of $\frac{x^{n-rm}\dot{x}}{\sqrt{a^{2m}-x^{2m}}}$ and $\frac{x^{n-\overline{r+1}.m}\dot{x}}{\sqrt{a^{2m}-x^{2m}}}$. Thus we have a general law of continuation, where the index of x without is increaſed by half the index under the vinculum.

V. Aſſume $\dot{F}=\frac{x^n\dot{x}}{x^m-b}=x^{n-m}\dot{x}+bx^{n-2m}\dot{x}+b^2x^{n-3m}\dot{x}+\&c.+\frac{b^rx^{n-rm}\dot{x}}{x^m-b}$, then $F=\frac{x^{n-m+1}}{n-m+1}+\frac{bx^{n-2m+1}}{n-2m+1}+\frac{b^2x^{n-3m+1}}{n+3m+1}+\&c.+W$, where W is put for the fluent of the laſt term. Now $\int x^n\dot{x}\sqrt{\frac{x^m-a}{x^m-b}}=\int\frac{x^n\dot{x}}{x^m-b}\times\sqrt{\overline{x^m-a}\times\overline{x^m-b}}=F\times\sqrt{\overline{x^m-a}\times\overline{x^m+b}}$

$-\int \mathrm{F} \times \frac{2mx^{2m-1}\dot{x} - \overline{a+b}\,.\,mx^{m-1}\dot{x}}{2\sqrt{\overline{x^m-a}\times\overline{x^m-b}}}$ = (by ſubſtituting for F its value in the latter quantity, and putting A, B, C, &c. for the co-efficients which ariſe therefrom) $\mathrm{F}\times\sqrt{\overline{x^m-a}\times\overline{x^m-b}} - \mathrm{A}\int\frac{x^n\dot{x}}{\sqrt{\overline{x^m-a}\times\overline{x^m-b}}} - \mathrm{B}\int\frac{x^{n-m}\dot{x}}{\sqrt{\overline{x^m-a}\times\overline{x^m-b}}} - \mathrm{C}\int\frac{x^{n-2m}\dot{x}}{\sqrt{\overline{x^m-a}\times\overline{x^m-b}}} - \text{&c.} - \int \mathrm{W}\times\frac{2mx^{2m-1}\dot{x} - \overline{a+b}\,.\,mx^{m-1}\dot{x}}{2\sqrt{\overline{x^m-a}\times\overline{x^m-b}}}$. Now $\frac{x^n\dot{x}}{\sqrt{\overline{x^m-a}\times\overline{x^m-b}}} = \frac{x^n\dot{x}}{x^m-a}\sqrt{\frac{x^m-a}{x^m-b}}$, $\frac{x^{n-m}\dot{x}}{\sqrt{\overline{x^m-a}\times\overline{x^m-b}}} = \frac{x^{n-m}\dot{x}}{x^m-a}\sqrt{\frac{x^m-a}{x^m-b}}$, $\frac{x^{n-2m}\dot{x}}{\sqrt{\overline{x^m-a}\times\overline{x^m-b}}} = \frac{x^{n-2m}\dot{x}}{x^m-a}\sqrt{\frac{x^m-a}{x^m-b}}$, &c. But

$$\frac{x^n\dot{x}}{x^m-a} = x^{n-m}\dot{x} + ax^{n-2m}\dot{x} + a^2x^{n-3m}\dot{x} + \text{&c.} + a^{r-1}x^{n-rm}\dot{x} + \frac{a^rx^{n-rm}\dot{x}}{x^m-a}$$

$$\frac{x^{n-m}\dot{x}}{x^m-a} = \qquad x^{n-2m}\dot{x} + ax^{n-3m}\dot{x} + \text{&c.} + a^{r-2}x^{n-rm}\dot{x} + \frac{a^{r-1}x^{n-rm}\dot{x}}{x^m-a}$$

$$\frac{x^{n-2m}\dot{x}}{x^m-a} = \qquad x^{n-3m}\dot{x} + \text{&c.} + a^{r-3}x^{n-rm}\dot{x} + \frac{a^{r-2}x^{n-rm}\dot{x}}{x^m-a}$$

&c. &c. &c.

Hence, by ſubſtitution, $\int x^n\dot{x}\sqrt{\frac{x^m-a}{x^m-b}} = \mathrm{F}\times\sqrt{\overline{x^m-a}\times\overline{x^m-b}} - \mathrm{A}\int x^{n-m}\dot{x}\sqrt{\frac{x^m-a}{x^m-b}} - \overline{\mathrm{A}a+\mathrm{B}}\times\int x^{n-2m}\dot{x}\sqrt{\frac{x^m-a}{x^m-b}} - \overline{\mathrm{A}a^2+\mathrm{B}a+\mathrm{C}}\times\int x^{n-3m}\dot{x}\sqrt{\frac{x^m-a}{x^m-b}} - \text{&c.} - \overline{\mathrm{A}a^{r-1}+\mathrm{B}a^{r-2}+\mathrm{C}a^{r-3}+\text{&c.}}\times\int x^{n-rm}\dot{x}\sqrt{\frac{x^m-a}{x^m-b}} - \overline{\mathrm{A}a^r+\mathrm{B}a^{r-1}+\mathrm{C}a^{r-2}+\text{&c.}}\times\int\frac{x^{n-rm}\dot{x}}{\sqrt{\overline{x^m-a}+\overline{x^m-b}}}$

$$-\int \mathrm{W}\times\frac{2mx^{2m-1}\dot{x} - \overline{a+b}\,.\,mx^{m-1}\dot{x}}{2\sqrt{\overline{x^m-a}\times\overline{x^m-b}}}.$$ But

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$-\int W \times \frac{2mx^{2m-1}\dot{x} - \overline{a+b}.mx^{m-1}\dot{x}}{2\sqrt{\overline{x^m-a}\times\overline{x^m-b}}}$ is $-W \times \sqrt{\overline{x^m-a}\times\overline{x^m-b}}$ $+b^r \times \int x^{n-rm}\dot{x}\sqrt{\frac{x^m-a}{x^m-b}}$; hence, by ſubſtituting this for the laſt term, it is manifeſt, that $-W \times \sqrt{\overline{x^m-a}\times\overline{x^m-b}}$ will be deſtroyed by the laſt term of $F \times \sqrt{\overline{x^m-a}\times\overline{x^m-b}}$ when we ſubſtitute for F its value; therefore, if we put $M = \frac{x^{n-m+1}}{n-m+1} + \frac{bx^{n-2m+1}}{n-2m+1} + \&c. + \frac{b^{r-1}x^{n-rm+1}}{n-rm+1}$, we have $\int x^n\dot{x}\sqrt{\frac{x^m-a}{x^m-b}} = M \times \sqrt{\overline{x^m-a}\times\overline{x^m-b}} - A\int x^{n-m}\dot{x}\sqrt{\frac{x^m-a}{x^m-b}} - \overline{Aa+B} \times \int x^{n-2m}\dot{x}\sqrt{\frac{x^m-a}{x^m-b}} - \overline{Aa^2+Ba+C} \times \int x^{n-3m}\dot{x}\sqrt{\frac{x^m-a}{x^m-b}} - \&c. - \overline{Aa^{r-1}+Ba^{r-2}+\&c.-b^r} \times \int x^{n-rm}\dot{x}\sqrt{\frac{x^m-a}{x^m-b}} - \overline{Aa^r+Ba^{r-1}+Ca^{r-2}+\&c.} \times \int \frac{x^{n-rm}\dot{x}}{\sqrt{\overline{x^m-a}\times\overline{x^m-b}}}$. Hence if the laſt two fluents be given, we have the general law of continuation up to the fluent of $x^n\dot{x}\sqrt{\frac{x^m-a}{x^m-b}}$.

The utility of finding fluents by continuation was manifeſt to Sir ISAAC NEWTON, who firſt propoſed it; and ſince his time ſome of the moſt eminent mathematicians have employed much of their attention upon it. The method which I have inveſtigated and exemplified in this Paper I offer as being entirely new; and at the ſame time it not only exhibits, at once, the general law up to the required fluent, but alſo appears, from ſome of the inſtances here given, to be more extenſive and convenient in its application than any method hitherto offered.

The general reſolution of the given fluxion into a ſeries of fluxions of the ſame kind, where the index of the unknown quantity without the vinculum keeps decreaſing or increaſing either by the index under or by half the index, has not, that I know of, before been given; which furniſhes us at once not only with a very eaſy method of *continuing* fluents, but alſo points out a very ſimple method of inveſtigating the fluent of the given fluxion *without* continuation. For if $\int\dot{A}=p+b\int\dot{B}+c\int\dot{C}+d\int\dot{D}+$ &c. $\int\dot{B}=p'+c'\int\dot{C}+d'\int\dot{D}+$ &c. $\int\dot{C}=p''+d''\int\dot{D}+$ &c. &c. &c. then if for $\int\dot{B}$, $\int\dot{C}$, &c. &c. we ſubſtitute their reſpective values, we ſhall get a general ſeries for $\int\dot{A}$ without continuation. The extent of any new method is, at firſt, ſeldom obvious; and how far that which is here propoſed may be ſucceſsfully employed in other caſes will beſt appear from its application. Different methods will always be found to have their uſes in particular caſes; for where one becomes impracticable another will often be found to ſucceed; and I hope that which is here offered will contribute ſomething towards facilitating the inveſtigation of fluents.

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XXVI. *Conjectures relative to the Petrifactions found in* St. Peter's *Mountain, near* Maeſtricht. *By* Petrus Camper, *M. D. F. R. S.*

Read July 6, 1786.

THE diſcovery of a great number of petrified bones about the year 1770, in the mountain of St. Peter at Maeſtricht, and particularly of large jaw-bones with their teeth, ſuggeſted to the late M. HOFFMAN, firſt Surgeon to the Military Hoſpital at Maeſtricht, a worthy member of ſeveral learned Societies, and a great admirer of natural hiſtory, the idea that theſe maxillæ belonged to crocodiles. This notion was ſpread by himſelf and his literary correſpondents through all Europe.

He did me the favour to ſend me, not only the hiſtory of thoſe petrifactions, but alſo ſeveral figures of the jaw-bones in queſtion, and of other bones, which were all intirely new to me, except ſome fragments of the bones of turtles. I diſcovered, however, at the very firſt ſight, the characteriſtical differences which diſtinguiſhed theſe bones from thoſe of crocodiles, of which I had at that time ſeveral in my collection.

His intention was to write upon this ſubject, and to ſend his eſſay, containing his reaſons for ſuppoſing theſe bones to belong to crocodiles, to the Royal Society; but I diſſuaded him, as a friend, from doing this, leſt he ſhould afterwards be under a neceſſity of retracting his opinion: and I ſent him a figure of the lower jaw of a crocodile, accurately done by my own hand, and ſoon after the ſkull and under jaw of a pretty large crocodile; which induced him to defer his deſign of writing about theſe antiquities of the old world, until he ſhould be better informed on the ſubject of cetaceous fiſhes.

Major DROUIN, of Maeſtricht, who made, about the ſame time, a collection of an infinite variety of corals, madrepores, alcyoniums, echinites, belemnites, ſhells, and petrified wood, from the ſame mountain and its environs, likewiſe procured a beautiful ſpecimen of two maxillary bones of the ſame incognitum, but with the inſides turned outwards; and this gentleman alſo ſuppoſed them to belong to the crocodile. A ſketch of this ſpecimen is to be found in M. BUCHOZ's *Dons de la Nature*, tab. 68. But the ſpecimen itſelf is now in TEYLER's Muſeum, at Haerlem, with the whole of Major DROUIN's collection.

Another ſtill more valuable and perfect ſpecimen is to be ſeen at the houſe of the reverend Dean GODDING, of which there is likewiſe a rough ſketch in M. BUCHOZ's *Dons de la Nature*, pl. 66. In this the greater part of both the upper and under maxillary bones is intire, and a bone, with ſmall teeth, belonging to the palate; by which it appears, the animal had not only teeth in the jaw-bones, but alſo in the throat, as ſeveral fiſhes have, but which are never found in the mouth of crocodiles.

Notwithſtanding all my endeavours to convince my friends, and afterwards M. DROUIN, and particularly the Dean, whoſe valuable and truly beautiful ſpecimens I ſaw in the year 1782, I never could prevail upon them to adopt my opinion, that theſe bones belonged to phyſeteres or reſpiring fiſhes. M. HOFFMANN, adhering cloſely to the Linnæan Syſtem, objected,

jected, that the phyſeteres had teeth only in the lower jaw-bone, whereas this foſſil monſter had them in both upper and lower maxilla. He did not ſeem to recollect, that φυσητὴρ ſignifies ſomething reſpiring, or breathing, and applied to fiſhes, *breathing fiſhes*; nor that the phyſeteres, according to the Linnæan ſyſtem, have ſmall teeth in the upper jaw-bone, though larger ones in the lower jaw, according to the obſervations of Dr. OTHO FABRICIUS, in his Fauna Groenlandica, p. 42. where he mentions the *macrocephalus*, and p. 45. where he ſpeaks of the *microps*.

In Auguſt 1782, I ſent M. GODDING, who had favoured me with a copy of his valuable ſpecimen, a full demonſtration of its being the head of a phyſeter, or breathing fiſh, Delphinus, or Orca, or under whatever genus it may be ranked, as having large teeth of the ſame ſize in both the maxillæ. But in vain; for he continues ſtill to call it a crocodile, as if its value depended upon the ſpecies of the animal.

The analogy of all the other marine bodies ſeems to make it ſtill more probable, that theſe large bones belong to the inhabitants of the ſea, and not of rivers. The large turtles, the numberleſs echinites, madrepores, ſhells, alcyoniums, belemnites, orthoceratites, and ſo on, are all ſea animals; and the crocodile would, in that caſe, be the only inhabitant of the rivers mixed with them.

The pretended crocodile found near Whitby, in Yorkſhire, Phil. Tranſ. vol. L. p. II. 1758, § 92. p. 688. and ibid. § 108. p. 786. is undoubtedly the ſkeleton of a Balæna.

§ 2. After the deceaſe of M. HOFFMAN, his family having offered the whole collection for ſale, I went in Auguſt 1782 to Maeſtricht on purpoſe to examine it; and I could not but greatly admire the richneſs and beauty of the collection, eſpecially that of the foſſil bones from St. Peter's mountain; but as the heirs did not conſider the expences neceſſary to tranſport the collection down the Maeſe, where each ſovereign puts an enormous duty upon every thing that paſſes through his territories, nor the ſmall number of perſons who were likely to purchaſe it, they rated the price ſo high that nobody choſe to bid for it.

The eldeſt daughter having at length become poſſeſſed of the whole, offered me the principal ſpecimens at a price I agreed to. Amongſt them were the duplicates I have already ſent to the Britiſh Muſeum, and with which the honourable Truſtees are perfectly ſatisfied. Theſe ſpecimens may ſerve likewiſe to aſcertain what I have ſaid about them, as being real fragments of phyſeteres, ſome of turtles, and the like, but not a ſingle one of any ſpecies of crocodile.

§ 3. The arguments for their being jaw-bones and vertebræ of fiſhes ſeem to be, firſt, the ſmoothneſs of theſe bones; and, ſecondly, the many holes by which the nerves go out at the ſide, and under each tooth, as is very evident in that beautiful ſpecimen now in the Britiſh Muſeum, on the outſide of which eleven holes are viſible, in the ſame manner as they are in the delphini, and more particularly in the lower jaw-bone of the cete, the Phyſeter macrocephalus, or pot-fiſh, cachalot, &c. Thirdly, the form of the teeth, which have ſolid roots, as in tab. XV. fig. 6. B, C, E, F, and the ſix teeth of tab. XVI. Fourthly, becauſe there are little teeth in the palate, as in Dean GODDING's ſpecimen. Fifthly, becauſe the vertebræ have the appearance of true cetaceous vertebræ, as in fig. 5. tab. XV. and in ſeveral beautiful and large ſpecimens now in the Muſeum. Several of theſe vertebræ were beſides intirely unknown to me, and not at all analogous to the vertebræ of the crocodile, deſcribed and repreſented by Dr. N. GREW.

§ 4. As I intended to viſit London in 1785, I flattered myſelf I ſhould ſtill find the ſkeleton of the great crocodile formerly at Greſham College, and be able to find out ſuch characteriſtic diſtinctions as ſhould be neceſſary to decide the queſtion. Dr. GRAY was ſo kind as to go with me to the lower apartments of the Britiſh Muſeum, where we found, though not without difficulty, the ſkeleton much neglected, ſpoiled, and deprived of ſeveral intereſting parts. I admired, nevertheleſs, the remainder of it, being infinitely pleaſed with the tranſverſe ſutures, tab. XV. fig. 1, 2. *a*, *b*. *e*, *f*. δ, ζ. by which not only thoſe of the neck and thorax, but thoſe of the loins alſo, are divided, and which I made a drawing of, as large as the life, the 20th of October, 1785, of which fig. 1. and 2. are very accurate copies.

I confeſs I had not obſerved that particular diviſion or ſuture in the ſkeleton of a ſmall crocodile, of thirteen inches, made by my youngeſt ſon; but after being apprized of it by the large ſkeleton in the Muſeum, of twelve feet four inches, Paris meaſure, on looking at my own when I returned home, I found them both alike, and that thoſe parts were not epiphyſes; of which, however, the tranſverſe proceſſes of the neck. fig. 1. *d*, *e*, *q*, *o*, *n*, *p*, have all the appearance, though there is no other epiphyſis to be obſerved in the reſt of the bones of that large ſkeleton.

When we compare the foſſil vertebra, fig. 5. with thoſe now in the Muſeum, we ſhall find the epiphyſes AB, CD, analogous to *a*, *b*, *c*, *d*, fig. 4. being the real epiphyſes in the vertebra of a young porpoiſe.

I procured, in London, the largeſt vertebræ of the neck of a turtle I could get, and prepared two of them as in fig. 3. in which, as along the back of that ſingular creature, I found the tranſverſe diviſions *a*, *c*, *d*, *f*: of all which I have not ſeen a ſingle inſtance amongſt the dorſal ſpinæ from St. Peter's mountain, one of which conſiſts of ſeven, another of twelve, and a third of fourteen vertebræ. Some of the vertebræ have, I acknowledge, an inferior proceſs, as in the crocodile, *l*, *m*, fig. 1. Of theſe I have ſent likewiſe two to the Muſeum. The oſtrich, and the turtle *Mydas*, have ſuch proceſſes, but no quadruped I know of.

The articulation of the vertebræ with each other, by the ſurfaces of the bodies themſelves, is intirely different, not only from that of the crocodile, but from that of all the cetaceous fiſhes I have ever ſeen: and I dare venture to aſſert, I have ſeen a great many, excluſive of thoſe in my collection. The anterior part of the Maeſtricht vertebræ is more or leſs triangular and hollow, as in fig. 5. C, D, L. The poſterior A B is convex. Both theſe ſurfaces are very ſmooth, as if they had been covered with a very thin cartilage, and moved one upon the other, without being united by an elaſtic lamella, as in all quadrupeds and cetaceous fiſhes; in which the vertebræ have on both the ſurfaces a round brim, or circular edge, *a*, *h*, *i*, *b*, by means of which the ligaments are connected, and a flat hollow ſurface within, as *b*, *i*, fig. 4. for the elaſtic pulp that is between them.

§ 5. The dentition is ſo ſingular in theſe foſſil jaw-bones that it deſerves a particular deſcription. In all quadrupeds, as in man, the teeth which appear firſt are all ſhed at a certain period of life, and in the mean time new ones are formed above, under, or at the ſides of the primordial or temporary

teeth,

teeth, but in different ſockets. The grinders are not all renewed, but in general three when there are ſix, and two when there are five. Nature, however, is not always uniform in this operation. Mr. JOHN HUNTER, a worthy Member of our Society, has given a very intereſting and complete natural hiſtory of the teeth, in which theſe obſervations are ſtated.

In the crocodile the ſucceeding or ſecondary teeth appear even when the animal's head is equal to two feet; that is, when it has acquired one-third of its uſual growth. When they grow too faſt, before the temporary tooth is ſhed, they perforate the ſide of the bone, at the part where they meet with the leaſt reſiſtance. Inſtances of this variety occur in the large crocodile's head, which is in my collection.

In all quadrupeds the enamel is, of the ſolid parts of the teeth, the firſt formed, making a cavity, in which the other bony ſubſtance is depoſited, and formed by lamellæ placed one within another, as is obſerved by Mr. JOHN HUNTER in the work already mentioned, p. 92. To this the root is added, which is filled in the ſame manner till the tooth is long enough to pierce through the gums.

But in the foſſil jaw-bones of St. Peter's mountain, a ſmall ſecondary tooth is formed, with its enamel and ſolid root at once, within the bony ſubſtance of the primordial or temporary tooth itſelf, as is to be ſeen in the ſmall fragment now in the Britiſh Muſeum, and in tab. XVI. A, B, C, D, E; which, by continuing to grow, ſeem to make by degrees ſufficient cavities in the bony roots of the primary teeth: but what becomes of them at laſt, and how they are ſhed, I am not able to gueſs. I have one in my collection, where the ſucceeding tooth is intirely formed within the center and ſubſtance of the primordial tooth. In the 6th figure (tab. XV.) a little oval cavity is obſervable, which has been the ſeat of a new or ſecondary tooth.

§ 6. The maxilla inferior of the incognitum, ſent by me to the Britiſh Muſeum, is a moſt magnificent ſpecimen, having fourteen teeth. A ſimilar one, ſomewhat longer (as it meaſures 3½ feet) in my own collection, has alſo fourteen. Another fragment of the left ſide, two feet long and eight inches broad, ſhews the primordial and ſucceeding teeth in the cleareſt manner.

The ſpecimen, of which I ſent a drawing (tab. XVI.) to the illuſtrious Preſident of our Society, Sir JOSEPH BANKS, is ſtill more uſeful to confirm the mode of dentition than any other I have in my muſeum.

§ 7. Several ribs and the phalanges of the toes of the fore-feet, a ſpecimen of which I ſent in a fragment from the ſame rock, of about a foot long and eight inches broad, may ſerve as another proof of the difference between theſe and the crocodile's toes, when compared with the ſtill valuable, though neglected, ſkeleton in the Britiſh Muſeum; which I am ſorry I could not make a drawing of, having been too much employed on other objects.

All theſe characteriſtic differences cannot fail to convince the learned Society of the truth of what I have aſſerted, about the animal theſe bones belonged to; for though we cannot determine exactly the ſpecies itſelf, yet I flatter myſelf the preceding obſervations evidently prove, that they did not belong to any animal of the crocodile kind.

§ 8. Another very beautiful ſpecimen, a foot and a half long, and about ten inches broad, I have been induced to add, becauſe it contains the anterior part of the ſcutum of a very large turtle. Of this Mr. JOHN HUNTER has an analogous bone from the ſame mountain in his valuable collection, but ſent to him under another name. I am convinced it belonged formerly to a turtle; firſt, becauſe I have from the ſame mountain the intire back of a turtle, four feet long and ſixteen inches broad, a little damaged at the ſides, and a pretty large fragment of another turtle, in my poſſeſſion. 2dly, Becauſe I have a ſimilar one, but ſo placed within the matrix as to ſhew the inſide, which is perfectly analogous to the inſide of that piece in the back of a large turtle I got in London, by the favour of Mr. SHELDON. 3dly, Becauſe I have amongſt theſe bones the lower jaw-bone of a very large turtle, of which the crura, though not intire, are ſeven inches long, and diſtant from one another ſix inches; the thickneſs is equal to 1¼ inch.

All theſe fragments prove the frequency of turtle bones amongſt the other foſſil bones found in the mountain near Maeſtricht.

Dr. MICHAELIS wrote to me ſome time ago, that the abovementioned fragment, in Mr. J. HUNTER's Collection, belonged to a bird; which I could hardly believe, as I never had ſeen in any collection whatſoever, either in London, Paris, Bruſſels, Gottingen, Caſſel, Brunſwic, Hanover, or Berlin, nor in my own country, any foſſil bone belonging to a bird. I know there is a ſmall one deſcribed in the Abbé ROZIER's Journal de Phyſique, for March 1782, which is at preſent in the collection of M. D'ARCET, at Paris. I expect alſo from Montmartre a ſmall leg of a petrified bird; but theſe are the only ones I have ever heard of, thoſe of Stonefield, near Woodſtock, being moſt undoubtedly of fiſhes. I think it is a circumſtance worthy the attention of the curious, that no human bones, and of birds but very few, have been hitherto found in a petrified ſtate, and belonging to the old world.

PETRUS CAMPER.

Klein Lankum, near Franeker,
June 18, 1786.

EXPLANATION OF THE PLATES.

TAB. XV.

Fig. 1, 2. Are vertebræ taken from the ſkeleton of the crocodile deſcribed by Dr. NEH. GREW, in his Catalogue of the Natural Rarities at Greſham College, p. 42. and p. 43.

a, b. c, f. δ, ζ. the bodies of the vertebræ; *a, b* of the fourth; *c, f* of the firſt vertebra of the neck; *β, z, t.* and *x, y, w.* the ſpinous proceſſes; *γ β.* and *s.* the aſcending; *t,* and *u v.* the deſcending proceſſes; *g, h, c, i. d, e, n, p, o, q.* the tranſverſe, united by cartilages to the bodies of the vertebræ. GREW calls them *oſſa mucronata.* The tranverſe proceſſes of the fourth vertebra being loſt, the roots of the mucronated proceſſes are very evident at *g h, i k.*

On the under part of theſe vertebræ are (*l* and *m*) proceſſes, ſimilar to thoſe we find in the vertebræ of the neck in turtles and birds. Not only the ſix poſterior but the five anterior vertebræ of the back are provided with ſuch proceſſes; of theſe, however, Dr. GREW makes no mention.

Fig. 2. Repreſents the ſeventh vertebra of the back; A, and C. are the aſcending and deſcending proceſſes, forming the articulations with the adjacent vertebræ; B. the tranſverſe proceſs, to which is united the rib FB. in B.; DE. the ſpinous proceſs; H, H, I. the body of the ſame vertebra.

Theſe figures are as large as the life, and made from the ſame ſkeleton, now in the Britiſh Muſeum. The whole length is equal to 12½ feet, Paris meaſure; the head equal to 2 feet; the neck equal to 1 foot; the trunk equal to 3 feet 8 inches; the tail equal to 5 feet 8 inches. The meaſurement given by Dr. GREW does not agree with mine; but he ſeems not to have taken it with great attention (p. 42.), for he makes uſe of the words *about*, *almoſt*, &c.

OBSERVATION. What ſtruck me was, the tranſverſe ſuture, *a*, *b*. *c*, *f*. *δ*, *ζ*. which divided the bodies of all the vertebræ of the neck, back, and loins. This diviſion ended with the os ſacrum, which was intire, as were alſo the vertebræ of the tail. Dr. GREW ſeems only to haven taken notice of the ſutures belonging to the tranſverſe proceſſes.

I have a ſmall ſkeleton of a crocodile equal to 13 inches, in which the 7 vertebræ of the neck, 12 of the back, and the 5 of the loins, are divided in the ſame manner as in the large ſkeleton in the Britiſh Muſeum. Thoſe of the os ſacrum and tail are without, and have no mark of an epiphyſis.

CONCLUSION. The tranſverſe diviſion of the vertebræ abovementioned is alſo peculiar to this animal; and there is no epiphyſis, as in other animals.

To be ſure of this, I diſſected and made a ſkeleton of the Lacerta Iguana, LINN. ſp. 26. perfectly well deſcribed by

MARCGRAF, *Hiſt. Braſ.* p. 236. cap. 11.; but I found no ſuch diviſions, though the animal was young, and though it had ſtill epiphyſes on the legs, &c. The neck conſiſts of 4 vertebræ, the back of 11, the loins of 9, the os ſacrum of 2, as in the crocodile; the tail of more than 60.

The diſſection of tortoiſes ſeemed to me of conſequence, at leaſt a more accurate inſpection of the vertebræ, particularly thoſe of the neck, as being analogous in ſome reſpects to thoſe of the crocodile, eſpecially in the ſtructure of the inferior proceſſes D, and E, with *l*, *m*, fig. 1.

Fig. 3. Repreſents two vertebræ of the neck of a pretty large turtle, natural ſize.

AB, BC. the bodies; L. and I. the aſcending, H. and T. the deſcending proceſſes; R. K. the ſpinous, *a*, *b*. *d*, *e*. the tranſverſe, and D. E. the inferior proceſſes.

a, *b*, *c*. *d*, *e*, *f*. the tranſverſe diviſion of theſe, ſimilar to that in the crocodile.

Fig. 4. A vertebra from the tail of a young phocæna or porpoiſe; in which *a*, *b*. is an orbicular plate, united by means of cartilage to the body of the vertebra *a*, *d*. which is provided with ſuch a one on both ſides, *a*, *b*. and *c*, *d*.

Thoſe bony lamellæ are the epiphyſes of the vertebræ, and are alike in all quadrupeds, to which claſs all the cetaceous fiſhes belong. When we conſider the ſtructure in general of theſe laſt, we find the hind legs only are wanting, and of courſe the oſſa innominata; but the oſſa pubis are very remarkable in all of them.

Fig. 5. Is a foſſil vertebra of the unknown animal, whoſe bones are ſo often met with in St. Peter's Mountain at Maeſtricht. A, B, C, D. is the body; C, I, K, E, F. the ſpinous proceſſes; C, K, I. the medullary canal, running under K, E, F, in a direction parallel to IF, and coming out again at F. The remaining marks of the lamellated epiphyſes I, D. and A, B. are evident proofs of the analogy between theſe and the vertebræ of the cetaceous fiſhes; and alſo of their want of reſemblance to the vertebræ of the crocodile, as will appear by comparing the firſt and ſecond figures with the fifth.

Fig. 6. Is a very accurate drawing of one of the foſſil teeth belonging to the ſame incognitum. A B C. is its point, of a lanceolated figure, whoſe edges B A, and A C, are dentated; B C. is the root, uneven, bony, fixed within the ſocket with D, G, F.; D, G, B, C. is covered with the gums; H, I. is an oval ſinuoſity, in which generally the ſecondary teeth are generated, as is ſeen in tab. XVI. repreſenting a fragment of the upper jaw-bone of the ſame incognitum, A, B, C, D, E.

The teeth in all the Phyſeteres and Delphini have ſolid roots, except in the young ones, in which they often have cavities to receive the blood-veſſels and nerves. But the crocodile has the teeth intirely hollow, as appears in

Fig. 7. in which the cavity Π, Δ, Θ, ſhews the difference between the crocodile's teeth and thoſe of the cetaceous and other fiſhes. This tooth is the anterior one of a large head of a crocodile, two feet long, and of the ſame ſize as that in the Britiſh Muſeum. A hollow tooth may notwithſtanding belong to a Phyſeter, as Dr. OTHO FABRICIUS obſerves in his Fauna Groenlandica, p. 44. when ſpeaking of the Phyſeter microps: of which he ſays, "Habet in maxilla inferiori dentes

22, utrinque 11 arcuatos, falciformes, *intus ad apicem uſque cavos*," within they are hollow to the very end.

T A B. XVI.

Fragmentum Maxillæ ſuperioris, lateris dextri capitis Phyſeteris incogniti, ex Monte St. Petri, Traj. ad Moſam. Origo dentium ſerotinorum ex ipſis radicibus ſolidis primo enatorum in quinque manifeſta eſt. Quæ ad dentitionem hanc ſingularem pertinent, ex figur. 2. Tab. Fragm. ſimilis ſed Maxill. inf. 12 Aug. 1784. peti debent.

Testudinis Marinae
Vertebrae e Collo:
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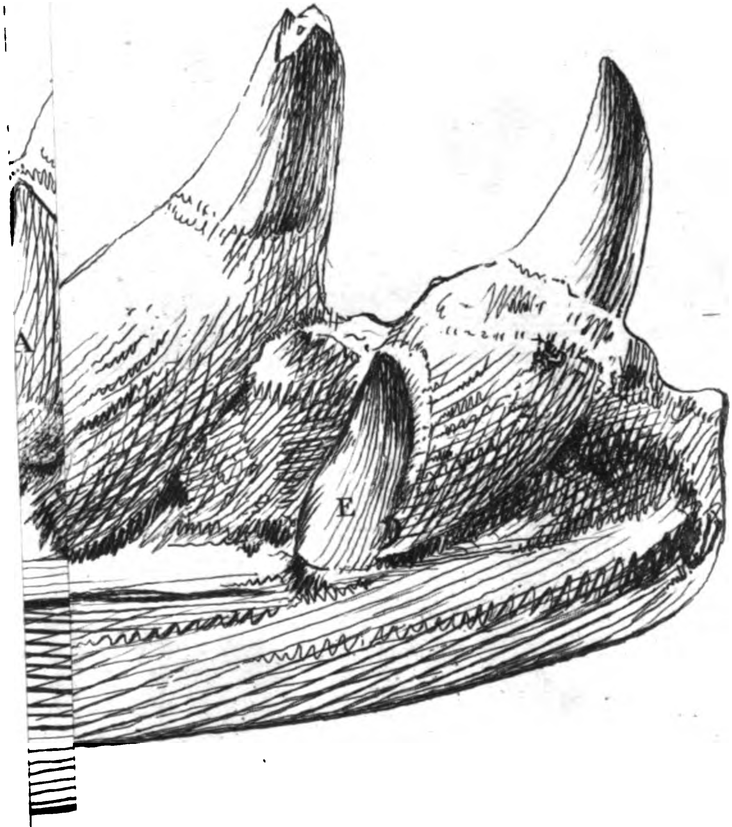

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XXVII. *Catalogue of One Thouſand new Nebulæ and Cluſters of Stars. By* William Herſchel, *LL.D. F. R. S.*

Read April 27, 1786.

THE following Catalogue, which contains one thouſand new Nebulæ and Cluſters of ſtars, is extracted from a ſeries of obſervations (or Sweeps of the heavens), which was begun in the year 1783, and which I am ſtill continuing till the whole be completed. As I may, perhaps, find an opportunity hereafter to publiſh theſe obſervations at full length, I ſhall now only mention ſuch circumſtances, relating to the inſtrument and apparatus with which they were made, as will be neceſſary to ſhew what degree of accuracy may be expected in the determination of the places of theſe Nebulæ and Cluſters of ſtars; and alſo to ſerve any aſtronomer, who wiſhes to review them, to form a judgment what inſtrument will ſuffice for this purpoſe.

The teleſcope I have uſed, as has been obſerved on a former occaſion*, is a Newtonian reflector of 20-feet focal length, and $18\frac{7}{10}$ inches aperture. The ſweeping power has been 157, except where another is expreſsly mentioned. The field of view 15′ 4″.

My eye-glaſs is mounted on that ſide of an octagon tube, which, in the horizontal poſition of the inſtrument, makes an angle of 45° with the vertical; having found, by experience, that this poſition, reſembling the ſituation of a reading deſk, is preferable to the perpendicular one commonly uſed in the Newtonian conſtruction.

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* Philoſophical Tranſactions, vol. LXXIV. p. 437.

In the preſent improved ſtate of the apparatus this teleſcope will, in general, give the relative place of an object by a ſingle obſervation true to within 1½ or 2 minutes of polar diſtance, and 4 or 6 ſeconds of time in right aſcenſion. But when there is an opportunity of repeating the obſervation, it will hardly differ a ſingle minute in the former, and ſeldom ſo much as 3 or 4″ in the latter. My apparatus, however, has not been equally perfect from the beginning; for, being from time to time adapted to the different views I had in ſweeping, it could only arrive to its preſent degree of perfection by many experiments and gradual improvements.

To begin a ſhort hiſtory of this 20-feet teleſcope. In the month of October of the already mentioned year I began to uſe it, being then mounted on its preſent ſtand, but with a lateral motion under the point of ſupport of the great ſpeculum, by which its direction could be changed about 15 degrees. It had alſo a kind of moveable gallery in front, about nine feet long, which permitted me to follow a celeſtial object near 15 degrees more; by which means I obtained a range of 30 degrees without moving the ſtand. The Newtonian form has the capital advantage of rendering obſervations equally commodious in all altitudes; I had therefore placed the inſtrument in the meridian, that I might view the ſtars in their moſt favourable ſituation.

When I had ſeen moſt of the objects I wiſhed to examine, I proceeded to the work of a general review of the heavens. The firſt method that occurred was, to ſuffer the teleſcope to hang freely in the center; then, walking backwards and forwards on the moveable gallery, I drew the inſtrument from that poſition by a handle faſtened to a place near the eye-glaſs, ſo as to make it follow me, and perform a kind of very ſlow oſcillations of 12 or 14 degrees in breadth, each taking up generally from 4 to 5 minutes of time. At the end of each oſcillation I made a ſhort memorandum of the objects I chanced to ſee; and when a new nebula or cluſter of ſtars came in my way, I made a delineation of the ſtars in the field of view, both of the finder and of the teleſcope, that it might ſerve me to find them again. This being done, the inſtrument was, by means of a fine motion under my hands, either lowered or raiſed about 8 or 10 minutes, and another oſcillation was then performed like the firſt. Thus I continued generally for about 10, 20, or 30 oſcillations, according as circumſtances would permit; and the whole of it was then called a *Sweep*, and as ſuch numbered and regiſtered in my journal.

When I had completed 41 Sweeps, the diſadvantages of this method were too evident to proceed any longer. By going into the light ſo often as was neceſſary to write down my obſervations, the eye could never return ſoon enough to that full dilatation of the iris which is abſolutely required for delicate obſervations. The difficulty alſo of keeping a proper memorandum of the parts of the heavens which had been examined in ſo irregular a manner, intermixed with many ſhort and long ſtops while I was writing, as well as the fatigue attending the motion, upon a not very convenient gallery, with a teleſcope in my hands of no little weight, eſpecially at the extremes of the oſcillations, where it made a conſiderable arch upwards, were ſufficient motives to induce me to look out for another method of ſweeping. And it is evident, that the places of nebulæ hitherto determined, which was till the 13th of December, 1783, muſt be liable to great inaccuracy. I therefore began now to ſweep with a vertical motion; and as this increaſed the labour of continually elevating and depreſſing the teleſcope by hand, I called in the aſſiſtance of a workman to do that part of the buſineſs, by which means I could obſerve very commodiouſly, and for a much longer time than before.

Soon after I removed alſo the only then remaining obſtacle to ſeeing well, by having recourſe to an aſſiſtant, whoſe care it was to write down, and at the ſame time loudly to repeat after me, every thing I required to be written down. In this manner all the deſcriptions of nebulæ and other obſervations were recorded; by which I obtained the ſingular advantage that the deſcriptions were actually writing and repeating to me while I had the object before my eye, and could at pleaſure correct them, whenever they diſagreed with the picture before me without looking from it.

In about half a dozen ſweeps, done according to this new way, I found that the ſtars of FLAMSTEED's Catalogue entered nearly at the time when they were expected; this ſuggeſted the poſſibility of converting my teleſcope into a tranſit inſtrument. By way of trial, Dec. 18, 1783, I began to uſe a watch, and noted the times of the tranſits of ſtars and nebulæ to the neareſt minute; and, this ſucceeding, Dec. 24, a ſidereal time-piece was introduced.

I found alſo that, by the turns of the handle which gave motion to the teleſcope, it was practicable, in a coarſe way, to aſcertain the difference of altitude between any two objects that paſſed the field of view; on which account, Dec. 30, I began to uſe an index-board, divided into inches, and marked with numbers, which, being placed behind the rope that moved the teleſcope, would point out at what altitude a certain index, affixed to the rope, was ſituated. My tackle of ropes and pullies was ſuch that, while the teleſcope traverſed an arch of two degrees, the mark on the rope paſſed over about 24 inches of the index-board: but the exact meaſure was always to be determined experimentally, as it varied according to the ſituation of the inſtrument. I perceived immediately that the quantity of rope uſed in the motion of the teleſcope would be much better obſerved by the aſſiſtant, if the index were brought within doors near the writing deſk: to effect this, I uſed a ſmall cord, which, being led off from the great one, was carried over a pulley into the obſervatory, ſo as to paſs over a ſet of numbers, which I now divided into ſuch parts as, in an equatorial ſituation of the inſtrument, would give nearly each equal to one minute.

It would exceed the limits of this Paper to enumerate the various trials I made to bring the right aſcenſion to greater perfection; ſuch as cauſing the tube ſometimes to hang inclining or rubbing againſt a perpendicular plane; at others, drawing it againſt the ſame by a ſmall weight, faſtened to a cord, paſſing over a ſide pulley, &c. I ſhall alſo paſs over the ſeveral changes in the form of the machine ſhewing the polar diſtance, which, for convenience ſake, was ſoon brought to an index moving over a dial, in the manner of a clock.

By way of directing the perſon who gives motion to the teleſcope, a ſmall machinery was added, which ſtrikes a bell at each extreme of the breadth of the ſweep, and is adjuſtable to any required number of turns of the handle.

In June, 1784, I introduced a ſmall quadrant of altitude, the uſe of which became ſoon after of the greateſt conſequence in determining the value of the numbers of the polar diſtance piece. Hitherto I had ſettled this value by cauſing a ſtar to paſs vertically through the field of the finder, which was very accurately limited to two degrees; but now I found, by many compariſons between the degree determined by the quadrant and by the finder, that I had generally under-rated the value of the numbers. Fortunately ſo many ſtars of FLAMSTEED'S Catalogue had been taken, that the numbers between their different polar diſtances were ſufficient to recover the value of the degree; but this occaſioned a laborious re-calculation of the places of all objects taken in near 300 ſweeps. The quadrant being once introduced, I carried the refinements of the determination, in high ſweeps where the ropes acted very unequally, ſo far as to aſcertain by it ſeparately the value of every 20 or 30 minutes throughout the whole breadth of a ſweep of two degrees, and the numbers were then accordingly caſt up by ſo many different tables calculated on purpoſe.

Being ſtill diſappointed in many inſtances, when, on a review of a nebula whoſe place I had before determined, I perceived a difference of 4 or 5 minutes in polar diſtance, I began at laſt intirely to new model the machinery of the polar diſtance piece, and on Sept. 24, 1785, completed one with the following capital improvements. My former piece ſhewed a ſet of numbers whoſe value differed in every ſituation of the teleſcope, and therefore required different and very extenſive tables to caſt them up in degrees and minutes. This ſhews at once both the degree and minute of the polar diſtance of every celeſtial object, without requiring any tables to caſt up numbers. In the next place, the conſiderable inaccuracy ariſing from the unequal tenſion of the great ropes, and their expanſion or contraction by moiſture or dryneſs, is intirely taken away; for now my index cord is contrived ſo as to go off from the front of the teleſcope itſelf, in the direction of a tangent to the arch it deſcribes when moving; by which means this cord will even ſerve as an hygrometer to ſhew the variations of the ropes that ſuſpend the teleſcope. If a ſhower of rain, for inſtance, ſhould ſhorten them ſo as to elevate the teleſcope 2, 4, or 6 minutes, which has happened ſometimes, notwithſtanding they have all been well ſaturated with oil, the index cord will immediately make the polar-diſtance-clock ſhew this effect of the rain, by pointing out an equal change on the dial. As to the variations of the cord itſelf, they are in the firſt place very trifling, ſince it conſiſts merely of a few threads of hemp, very looſely twiſted, well oiled, and always equally ſtretched; but eſpecially theſe variations are of no conſequence, as they are ſo eaſily to be diſcovered by the check of the quadrant of altitude affixed to the teleſcope, or the ſucceſſive tranſits of known ſtars, and may either be immediately corrected by the adjuſtable hand of the polar diſtance dial, or be left to be accounted for afterwards.

The improvement of the right aſcenſion has not been leſs attended to; and the Royal Society having kindly intruſted me with an excellent time-piece, I ſucceeded at laſt by means of the addition of the following apparatus. Againſt the ſide of the tube is fixed a vertical iron plate, and the point of ſuſpenſion of the teleſcope is diſpoſed ſo as to permit this plate to be juſt in contact with a roller which remains fixed during the time of a ſweep. There is alſo a conſiderable ſpring applied on the oppoſite ſide, in ſuch a manner as, by always exerting a preſſure nearly uniform, to cauſe the iron plate to rub againſt the fixed roller as the teleſcope ſweeps up and down. By this means I have frequently, in very ſtormy weather, obſerved many hours without finding my time materially affected, and the corrections will ſeldom, in accurate obſervations, exceed a few ſeconds.

To thoſe who are accuſtomed to the accuracy of tranſit inſtruments in regular obſervatories, this teleſcope, notwithſtanding the above-mentioned improvements, may perhaps appear far from being brought to perfection; but they ſhould recollect the ſize of the inſtrument as well as its extenſive uſe, ſince I can not only follow any object for near a quarter of an hour, without diſturbing the ſituation of the apparatus, but can at pleaſure, in a few minutes, turn it to any part of the heavens, and view a celeſtial object whereſoever it may chance to be ſituated, even the zenith not excepted.

From this account it will be underſtood, that the places of a few of the nebulæ and cluſters of ſtars, determined before the 13th of December, 1783, may be faulty in right aſcenſion as far as 1′ of time, and in polar diſtance to 8 or 10′ of ſpace. Afterwards the errors will be found to become gradually leſs conſiderable till the latter end of the year 1784, when, I ſuppoſe, they will ſeldom exceed half that quantity. From that period to Sept. 24, 1785, they will diminiſh, and probably not often amount to ſo much as 3 or 4′ in polar diſtance, and 10 or 12″ in right aſcenſion. And now I flatter myſelf that all places, determined ſince the laſt mentioned time, will generally be true to a very ſmall quantity; ſuch as 4 or 6″ in right aſcenſion, and 1½ or 2′ in polar diſtance, and often much nearer.

Some of the nebulæ in that part of the heavens which, in a former Paper, I have called the ſtratum of Coma Berenices, are indeed ſo crowded that there was no poſſibility of taking them all in the center of the field of view, and a ſomewhat leſs degree of accuracy may therefore be expected; but having uſed myſelf by very frequent eſtimations of the parts of the field of view to judge of their value in time as well as in ſpace, I corrected

I corrected this defect at the moment of obſervation by affixing to the tranſits of theſe excentric nebulæ ſuch proper marks of *plus* or *minus* in right aſcenſion and polar diſtance as I judged would bring them to a central obſervation. A ſimilar method, well known to good aſtronomers in eſtimating their tenths of ſeconds by the proportional ſpace over which the ſtars move in their meridian paſſage, makes it unneceſſary to expatiate on the degree of accuracy that long practice enables us herein to obtain.

If, however, I had been willing to delay giving this catalogue till, by a repeated review of the heavens, the places had been more accurately determined, the work would undoubtedly have been more perfect; but whoever conſiders that it requires years to go through ſuch obſervations will perhaps think with me, that it is the beſt way to give them in their preſent ſtate, if it were but to announce the exiſtence of ſuch objects by way of inducing other aſtronomers alſo to look out for them. Another motive for not delaying this communication is to ſhew that my late endeavours to delineate the conſtruction of the heavens have been guided by a careful inſpection of them; and, probably, a catalogue which points out no leſs than one thouſand inſtances of ſuch ſyſtems as thoſe are into which I have ſhewn the heavens to be divided, will conſiderably ſupport what has been ſaid on this ſubject in my two laſt Papers.

When the diurnal motion of the earth was firſt maintained, it could not but greatly add to the reception of this opinion when the teleſcope expoſed to our view Jupiter, Mars, and Venus, revolving on their axes*; and if theſe inſtances of the ſimilar condition of other planets ſupport the doctrine of the diurnal motion, the view of ſo many ſidereal ſyſtems, ſome of which we may diſcern to be of a moſt ſurpriſing extent and grandeur, will in like manner add credit to what I have propoſed with regard to the condition of our ſituation within a ſyſtem of ſtars: for, to the inhabitants of the nebulæ of the preſent catalogue, our ſidereal ſyſtem muſt appear either as a ſmall nebulous patch; an extended ſtreak of milky light; a large reſolvable nebula; a very compreſſed cluſter of minute ſtars hardly diſcernible; or as an immenſe collection of large ſcattered ſtars of various ſizes. And either of theſe appearances will take place with them according as their own ſituation is more or leſs remote from ours.

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* To theſe may now alſo be added Saturn, on whoſe body I have, in the year 1780, ſeen ſeveral belts, with ſpots that changed their ſituation in the courſe of a few nights.

In the diſtribution of the nebulæ and cluſters of ſtars into claſſes, I have partly conſidered the convenience of other obſervers: thus, in the firſt claſs, the degree of brightneſs of the nebulæ has been the leading feature, as moſt likely to point out thoſe which their ſeveral inſtruments may give them expectation to reach. The firſt claſs, therefore, contains the brighteſt of them; the ſecond, thoſe that ſhine but with a feeble light; and in the third are placed all the very faint ones. Beſides this general diviſion, I have added a fourth and a fifth claſs, which contain nebulæ that, on different accounts, ſeemed to deſerve a more particular deſcription than I had allotted to the three former diviſions.

The cluſters of ſtars are ſorted by their apparent compreſſion, in the manner of my former Catalogues of double, treble, and multiple ſtars; ſo that the cloſeſt and richeſt cluſters take up the firſt claſs; the brighteſt, largeſt, and pretty much compreſſed ones, the ſecond; and thoſe, which conſiſt only of ſcattered and leſs collected large ſtars, are put into the laſt.

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In every claſs the order of time when the nebulæ and cluſters of ſtars were diſcovered, or firſt obſerved with my 20-feet teleſcope, has been followed; and that I might deſcribe all theſe objects in as ſmall a compaſs as could well be done, I have uſed ſingle letters to expreſs whole words, an explanation of which, with an example of the manner of reading thoſe letters, is given. It ſhould be obſerved, that all eſtimations of brightneſs and ſize muſt be referred to the inſtrument with which the nebulæ and cluſters of ſtars were ſeen; the clearneſs and tranſparency of the atmoſphere, the degree of attention, and many more particular circumſtances, ſhould alſo be taken into conſideration; ſo that probably ſome of the nebulæ which I have called very bright, and very large, may only be juſt perceivable, as very ſmall faint patches, in many of our beſt common teleſcopes.

The Identity of each nebula in this catalogue has been well aſcertained by a projection on a proper map, made on purpoſe, which pointed out all other nebulæ near its place, and thus afforded the means of a rigorous examination. When, therefore, ſeveral nebulæ are found within the limits of the accuracy with which my teleſcope can diſcriminate them, in different nights, it may be concluded, that they were ſeen either at once in the ſame field of view, or otherwiſe in immediate ſucceſſion during the ſame ſweep.

In the ſame manner theſe nebulæ have been compared with thoſe that are contained in the two volumes of the *Connoiſſance des Temps*, for the years 1783 and 1784, of which none have been inſerted in this catalogue. It was indeed eaſy enough to diſtinguiſh the nebulæ of that excellent collection from thoſe of mine which in ſeveral places are very near them: The quantity of good light in my teleſcope having enabled me, even in bright moon-light nights, to ſee occaſionally ſome of the moſt feeble of the former, when the latter could not by any means be perceived.

Perhaps it will not be diſpleaſing to thoſe who may look out for ſome of the objects contained in this catalogue, to know that the pictures which were given in a former Paper, repreſenting the various ſhapes and appearances of ſeveral nebulæ, have been actually taken from nature, by Drawings made of them while I had them in view; I have therefore added a reference to theſe figures, as the deſcriptions of the originals which they repreſent occur in their order in the catalogue.

Arrangement of the columns, and explanations of the abbreviations.

The firſt column contains the claſs and number of the nebulæ.

In the ſecond are the dates when the nebulæ were firſt obſerved.

The third column contains the ſtar, or other object, by which the place has been determined.

In the fourth column the letter p or f ſhews that the nebula is either preceding or following the ſtar.

In the fifth is the time, in ſidereal minutes and ſeconds, by how much it precedes or follows the ſame ſtar.

The letter n or ſ, contained in the ſixth column, denotes that the nebula is north or ſouth of the determining ſtar.

In the ſeventh is the quantity, in degrees and minutes, by how much the nebula is more north or more ſouth than the ſame ſtar.

The eighth column contains the number of obſervations that have been made of each nebula; and it is to be noted, that the determination of the place is generally taken from the laſt obſervation, on account of the more perfect ſtate of the teleſcope.

The ninth column, or remaining ſpace, contains the deſcription of the nebulæ, by means of ſingle letters, or now and then a few words added to them.

The abbreviations are to be underſtood as follows.

B.	Bright.	v.	very.
F.	Faint.	c.	conſiderably.
L.	Large.	p.	pretty.
S.	Small.	e.	extremely.

Of theſe letters I have compoſed vB. cB. pB. pF. vF. eF. vL. pL. pS. vS. eS.; all which require no farther explanation.

R.	Round.	l.	a little.
E.	Extended.	i.	irregularly.
M.	in the middle.	g.	gradually.
b.	brighter.	ſ.	ſuddenly.
m.	much.		

When theſe are joined we have iR. mE. lE. bM. gbM. ſbM. mbM. lbM. glbM. gmbM. ſmbM., and by taking in ſome of the former letters BM. vBM. cBM.; where no other remark will be neceſſary than that writing for inſtance bM, or brighter in the middle, it is intended to expreſs, that a nebula, which is faint at the borders, is leſs ſo towards the middle. And theſe degrees of brightneſs happening ſometimes to be ſo well united from the moſt imperceptible border to a very luminous center, I have, on ſuch occaſions, uſed the expreſſion vgmbM, or very gradually much brighter in the middle.

r.	reſolvable.	m.	milky.

er. (joined) eaſily reſolvable.

iF. (joined) of an irregular figure.

C. Cometic, or reſembling a teleſcopic comet.

N. having a Nucleus, or bright compreſſed ſpot.
l, b, or d. (joined to minutes) long, broad, or diameter.
ſt. a ſtar. ſtars.
n. north. north of.
ſ. ſouth. ſouth of.
p. preceding. np. north preceding. ſp. ſouth preceding.
f. following. nf. north following. ſf. ſouth following.
betw. between. ver. 240. verified by a power of 240.
bran. branches.
che. chevelure.
mer. in the direction of the meridian.
par. in the direction of the parallel of declination.
np ſf. in a direction from north preceding to ſouth following.
ſp nf. in a direction from ſouth preceding to north following.

Example. I. 13. 22. 69 Leon. p. 7. 57. n. 0. 2. 3. vB. mE. mer. ſmbM. 7 or 8′ l.

13th nebula of the 1ſt claſs. Feb. 22, 1784. It precedes the 69th Leonis of Flamsteed's Catalogue 7′ 57″ in time, and is 0° 2′ more north than that ſtar. 3 obſervations. Very bright, much extended in the direction of the meridian of the nebula, ſuddenly much brighter in the middle 7 or 8′ in length.

I. 32. . . . . p. 5. 11. n. 0. 28. 3. cB. S. BN. and 2vF bran. 32d nebula of the firſt claſs. April 13, 1784. It precedes the 31ſt (or 1ſt d) Virginis of Fl. Cat. 5′ 11″ in time, and is 0° 28′ more north than that ſtar. 3 obſervations. Conſiderably bright, ſmall, having a bright nucleus, and two very faint branches.

Firſt claſs. Bright nebulæ.

I.	1783	Stars.		M.	S.		D.M.		Ob.	Deſcription.
1	Dec. 19	82 (δ) Ceti	f	2	17	n	0	8	7	cB. cL. iF. bM.
2	—	3 Leonis	p	18	7	ſ	1	12	5	cB. cL. vgbM. N. R.
3	—	34 Sextant	p	28	55	ſ	0	13	4	cB. pL. C. mbM.
4	—	– –	p	28	27	ſ	0	10	4	cB. pL. C. mbM.
5	30	81 Leonis	p	2	42	n	0	7	2	B. pS. iR. bM. r.
	1784									
6	Jan. 19.	64 Virginis	f	33	56	ſ	0	1	3	vB. pL. gmbM.
7	23	49 Leonis	f	126	45	ſ	0	40	1	vB. L. R. The place inac.
8	—	32 (2 *d*) Virg	f	2	50	n	0	48	5	cB. pL. iR. mbM. r.
9	24	10 (*r*) Virg	f	3	12	ſ	0	35	4	cB. E. np ſf. N and 2 bran. 3′l.
10	—	– –	f	33	37	n	0	4	4	vB. pL. lE. gmbM. 2′l. 1½′b.
11	Feb. 15	5 Comæ Be.	p	1	30	ſ	2	11	1	B. pL. lE. bM. m.
12	19	6 Comæ	f	9	12	ſ	0	9	2	B. pS. R. BM. r.
13	22	69 Leonis	p	7	57	n	0	2	3	vB. mE. mer. ſmbM. 7 or 8′l. Fig. 11.
14	—	29 (γ) Virg	f	0	43	n	1	23	2	cB. cL. mE. near par 3 or 4′l.
15	—	– –	f	3	23	n	0	58	2	cB. mE. ſp nf. ſbM. 4 or 5′ l.
16	—	– –	f	10	34	n	0	13	2	cB. vL. iF. vgmbM.
17	Mar. 11	46 (*i*) Leo	f	15	50	ſ	1	32	5	The 2 p of 3. Both vB. cL. mbM. C. II. 41. Fig. 4.
18			f	16	18	ſ	1	29	5	
19	14	11 Comæ	p	10	30	n	0	46	1	vB. pL. gbM.
20	15	73 (*n*) Leonis	f	8	52	ſ	1	57	2	vB. mE. nearly par.
21	—	– –	f	25	31	ſ	1	49	3	vB. cL. R. gmbM.
22	—	34 Virginis	p	22	24	ſ	0	17	2	cB. pS.
23	—	– –	p	18	24	ſ	0	19	2	B. S. mE.
24	—	30 (ρ) Virg	p	1	42	ſ	0	5	2	vB pL. r. near 2 Bſt.
25	—	34 Virginis	f	4	45	ſ	0	40	1	B. S. in a line with 2 ſt.
26	19	52 (K) Leonis	p	3	45	ſ	2	9	1	cB. pL. not R. mbM.
27	Apr. 8	46 (*i*) Leonis	f	18	47	ſ	0	43	3	vB. BNM. and 2 F bran. np ſf.
28	—	34 Virginis	p	19	36	n	1	8	2	One of two, at 4 or 5′. diſt. B. cL.
29	12	73 (*n*) Leonis	p	1	9	ſ	0	30	3	vB. cL. E par. mbM.
30	13	31 (1 *d*) Virg	p	17	41	n	0	32	2	vB. cL. lE. iF.
31	—	31 (1 *d*) Virg	p	8	0	n	0	37	1	vB E. mbM. r. betw. 2 Bſt.
32	—	– –	p	5	11	n	0	28	4	cB. S. BN and 2 vF. bran.
33	15	9 (*o*) Virgin	f	3	12	n	1	39	1	B. L. mE. mbM. r.
34	—	59 (*e*) Virgin	f	20	42	ſ	0	34	2	vB. cL. E np ſf. SBN.
35	17	34 Virginis	p	31	42	n	1	5	1	B. vmE. vBM. 9 or 10′ l.
36	—	– –	p	11	24	n	0	20	1	Two. Both B. S. lE.
37										
38	18	32 (2 *d*) Virg	p	11	36	n	0	0	1	B. vL. mE. mbM.

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I.	1784	Stars.		M.	S.		D.	M.	Ob.	Deſcription.
39	Apr. 24	51 (θ) Virg	p	21	36	ſ	0	14	1	vB. vL. ſmbM. rN.
40	—	— —	p	5	48	n	0	2	1	cL. vBSNM.
41	25	28 Virginis	f	9	24	n	1	2	1	B. L. iR. lbM.
42	—	26 (χ) Virgin	f	30	27	n	0	8	2	cB. L. iR. vgbM.
43	May 9	49 (g) Virgin	p	28	6	ſ	0	51	1	E. vBM. 5 or 6′ l.
44	21	51 (e) Ophiu	f	7	18	n	0	0	2	cB. pL. N.
45	24	43 Ophiuchi	p	6	36	n	0	4	2	B. R vgmbM.
46	—	— —	f	0	54	n	1	46	1	pB. cL. R. BM. r.
47	June 16	1 (m) Aquilæ	f	17	48	ſ	0	33	1	B. vL. iF. er. ſt viſible.
48	17	43 (d) Sagit	p	114	6	n	1	44	1	B. L. R. gbM. er.
49	24	10 (γ) Sagit	p	2	18	n	0	23	1	B. pL. bM. r.
50	—	19 (δ) Sagit	f	3	0	ſ	0	33	1	cL. R. vBM. m.
51	July 12	22 (λ) Sagit	f	3	12	ſ	0	13	1	cL. R. vBM. er.
52	Aug. 21	17 Delphini	f	6	0	n	2	24	4	vB. S. R. gmbM. r.
53	Sept. 5	66 (υ) Cygni	f	78	6	ſ	0	51	2	vB. cL. mE. mbM. r.
54	Oct. 5	35 (ν) Andr	f	12	44	ſ	2	50	1	B. cL. R. mbM. Place inacc.
55	19	66 Pegaſi	p	17	59	n	0	2	3	cB. mE. mer. gbM. 4′l. 2′b.
56 57	Nov. 16	4 (λ) Leonis	f	0	46	ſ	1	29	1	Two, at 1′ diſtance. Both cB. cL. appear like one mE.
58	17	19 Eridani	f	5	9	ſ	1	22	2	B. S. lE. mbM.
59	20	15 (ι) Navis	f	64	18	n	0	21	1	S. cBM. lE. m.
60	Dec. 9	19 Eridani	p	6	51	n	0	16	1	vB. S. lE. mbM.
	1785									
61	Jan. 6	6 Sextantis	p	8	42	n	0	31	2	vB. S. iF. 1′ nſcBſt.
62	10	55 (ζ) Ceti	p	0	25	n	0	37	2	cB. pL. E. bM.
63	—	80 Ceti	f	5	12	ſ	0	25	1	B. R. mbM. 1′ d.
64	—	8 (1 ρ) Erid	p	15	9	n	0	2	2	vB. pL. lE. mbM.
65	Feb. 7	31 Crateris	f	23	30	n	0	52	1	vB. pL. iR. bM. like 2 N.
66	8	12 Hydræ	f	25	2	ſ	1	7	1	B. vS. iF. mbM.
67	—	8 (η) Corvi	p	37	17	n	2	10	3	cB. pL. iF. mbM. 2 or 3′ d.
68	—	53 Virginis	p	12	40	n	1	4	1	cB. iR. mbM.
69	—	— —	p	11	4	n	1	34	1	cB. pL. iR.
70	Mar. 5	106 Virginis	f	1	2	n	0	54	1	vB. cL. iF. vgbM.
71	—	19 (δ) Libræ	p	0	3	n	1	4	2	cB. vS. b towards f ſide.
72	13	23 Leonis min	f	13	7	n	0	13	1	cB. cL. E. mbM.
73	—	13 Can. vena	p	50	17	ſ	0	22	1	vB. S.
74	—	13 Can. vena	p	43	5	ſ	1	11	1	cB. R. mbM.
75	—	— —	p	40	35	ſ	1	9	1	vB.
76	—	— —	p	38	3	ſ	0	52	1	cB. L. E.
77	—	— —	p	34	15	n	0	23	1	vB. L. broadly E. bM.
78	April 3	27 Urſæ	f	7	46	n	0	4	1	vB. cL. vſmbM.
79	—	— —	f	33	52	n	1	17	1	cB. pL. R. vgmbM.
80	—	— —	f	67	10	n	0	46	1	cB. S. i elliptical.
81	6	41 Leonis min	p	0	6	n	1	40	2	cB. cL. m. juſt p 2 ſt.

I.

I.	1785	Stars.		M.	S.		D.	M.	Ob.	Deſcription.
82	Apr. 6	14 (*b*) Comæ	p	37	40	ſ	0	14	2	cB. pL. lE. mer. vgbM.
83	—	21 (*g*) Comæ	f	0	10	n	1	12	1	cB. pl, iR mbM.
84	—	– –	f	19	34	n	0	55	1	cB iR. ſBM. m. 7 or 8′ d.
85	10	40 Comæ	f	5	9	n	0	18	1	cB. pL.
86	11	39 Leonis min	p	13	14	n	0	59	1	cB. pL. mbM. brightneſs lE.
87	—	44 Leonis min	f	9	30	n	1	1	1	vB. vL. gbM.
88	—	– –	f	13	30	n	0	1	1	cB. cL. iR. mbM.
89	—	14 (*b*) Comæ	p	8	18	n	0	55	1	vB. S. lE.
90	—	– –	p	6	30	n	1	57	1	The np of 2 cB. pL R. II. 377.
91	—	15 (*c*) Comæ	f	1	10	n	0	19	1	vB. E. par. pBLN. and 2 bran.
92	—	– –	f	9	8	ſ	0	19	1	vB. vL. mE. np ſf. 10 or 12′ l. 4 ſt. in it.
93	—	31 Comæ	f	2	56	n	1	24	1	cB. pL.

Second claſs. Faint nebulæ.

II.	1783	Stars.		M.	S		D.	M.	Ob.	Deſcription.
1	Oct. 28	41 Aquarii	p	15	::	ſ	½	::	1	F. cL. mE. bM. er.
2	30	24 (α) Piſ. auſt	f	14	40	n	1	2	3	pB. S. iF. mbM.
3	Dec. 13	17 (1 φ) Ceti	p	9	::	n	2	::	2	F. L. mE. between 2 cBſt.
4	—	41 Ceti	f	15	13	n	0	37	6	pB. pS. R. mbM. C.
5	18	82 (δ) Ceti	p	0	5	n	0	46	8	pB. S. lE. bM.
6	—	– –	p	1	::	n	½	::	1	S. C, between 2 L and 1 S ſt.
7	19	45 Eridani	p	1	13	n	0	54	3	F. pL. iR. vlbM.
8	—	44 Eridani	f	2	11	ſ	0	41	4	Two. The firſt. F. S. r.
9			f	2	18	ſ	0	42	4	The ſecond, F. vS. r.
10	24	88 (γ) Pegaſi	p	13	38	ſ	0	23	2	F. pL. E. ſp nf. bM. r.
11	30	6 Comæ	f	1	24	n	0	24	2	F. pL. nearly R. r.
12	—	27 Comæ	p	3	15	ſ	0	9	2	pB. cL. lE. mbM. r.
	1784									
13	Jan. 18	78 (ι) Leonis	f	6	18	ſ	1	10	2	pB. pL. mbM. r.
14	—	3 (ν) Virginis	f	2	20	n	1	22	1	lE. not C.
15	—	20 Virginis	f	4	12	ſ	0	42	2	F. pL. pR.
16	23	56 Leonis	p	0	32	n	1	32	4	F. vS. nearly R.
17	—	9 (*o*) Virginis	f	11	54	ſ	1	33	3	F. pl. E. followed by III. 91.
18	—	31 (1 *d*) Virg	p	12	28	n	1	1	2	F. S.
19	—	I. Claſs 7 Neb.	p	0	0	ſ	0	12	1	F. pS. R.
20	—	31 Bootis	p	118	45	ſ	0	38	2	vS.
21	—	32 (2 *d*) Virg	f	9	28	n	0	22	4	pB. pL. b towards the p ſide.
22	—	31 Bootis	p	80	15				1	F. vS.
23	24	75 Leonis	f	70	0	n	0	26	2	F mE.
24	—	– –	f	97	0	n	0	7	2	F. pL.
25	—	78 Virginis	p	7	50	ſ	1	34	4	pB. cL. nearly R. mbM.

II.

II.	1784	Stars.		M.	S.		D.M.	Ob.	Deſcription.
26	Jan. 28	11 (*s*) Virg	f	18	0	n	0 45	1	pB. cL. b towards the f ſide.
27	30	31 Bootis	p	9	6	ſ	0 2	3	F. pL. R. lb not M.
28 29	Feb. 15	41 (γ) Leonis	f	3	45	n	0 18	1	Two, about 2′ aſunder. Both F. cL. R. Fig 3.
30	—	68 (δ) Leonis	f	6	30	ſ	2 23	1	pB. r.
31	22	29 (γ) Virg	p	2	31	n	0 55	2	pB. cL. lE. par. r.
32	23	84 (τ) Leonis	p	6	30	n	0 7	1	pB. vS. bM.
33	—	– –	p	6	0	n	0 20	1	pB. pL. R. bM.
34	—	60 (σ) Virgin	p	51	23	ſ	1 27	4	F. S.
35	—	16 (*c*) Virgin	f	8	45	n	0 15	2	pB. mbM.
36	—	60 (σ) Virgin	p	46	5	ſ	1 29	3	F vL. iR. bM. 6′ l 4′ b.
37	—	16 (*c*) Virgin	f	13	15	ſ	0 40	2	pB. E. np ſf. mbM.
38	—	35 Virginis	p	5	30	ſ	0 53	2	pB. pL. iF. r.
39	—	– –	p	3	0	ſ	0 20	1	pB. contains 2 ſtM.
40	Mar. 11	6 (*h*) Leonis	f	1	43	n	0 27	3	F. pL. lbM.
41	—	46 (*i*) Leonis	f	16	30	ſ	1 35	4	The f of 3. F. E.
42	—	78 (ι) Leonis	p	14	0	n	0 12	1	F. S.
43	12	36 (ζ) Leonis	p	3	10	ſ	0 40	2	pB. cL. iF.
44 45	—	20 Leonis	f	28	15	n	0 48	1	Two. Both. F. E. lbM. r.
46	—	68 (δ) Leonis	p	37	30	n	1 29	1	pB. S. r.
47	—	54 Leonis	p	2	26	ſ	1 49	2	pB. pL. lE. r. 3 or 4 ſt in it.
48	14	85 (l) Gemin	f	56	45	ſ	0 42	1	pB. pL. lbM. contains 1 ſt.
49	—	86 Leonis	p	15	0	ſ	0 19	1	vgbM. r.
50 51	—	– –	p	13	30	ſ	0 22	1	Of three that M. pB. cL. R. bM. That to the n. S. R. bM. III. 27.
52	—	– –	p	10	0	ſ	0 5	1	pB. S. lE. bM.
53	—	81 Leonis	p	1	36	n	1 19	2	F. E. r.
54	—	85 Leonis	p	1	24	n	1 29	2	F. S. R.
55	—	11 Comæ	f	4	45	n	0 24	1	The f. of 2. r. See Note.
56	—	25 Comæ	p	8	30	n	0 1	2	pL. iR. bM. 2 or 3′ d.
57 58	15	5 (ξ) Leonis	p	7	15	n	0 18	1	Two, diſtant 1′ np ſf. The p. pS. lbM. r. The f. pL. lbM. r.
59	—	– –	f	26	30	ſ	0 53	1	vS. C. in a row with 2 F and 1 Bſt.
60	—	12 (*t*) Virg	p	5	15	n	0 10	1	F. S.
61 62	—	34 Virginis	p	26	12	ſ	0 31	2	Two. nearly par. The firſt F pL. E. The ſecond F. pL. R.
63	—	– –	p	24	42	ſ	0 9	2	F. pL. mE.
64	—	12 (*t*) Virg	f	11	45	n	0 57	1	F. vS.
65	—	– –	f	14	45	n	0 50	1	pB. not vS.
66	—	30 (ρ) Virg	p	9	30	n	0 56	1	pB.
67	—	– –	p	8	30	n	1 6	1	pB. vS.
68	—	34 Virginis	p	10	24	ſ	0 36	2	pB.
69	—	30 (ρ) Virg	p	0	48	ſ	0 4	2	pB. pL. R. mbM. r.

II.

II.	1784	Stars.		M. S.		D.M.	Ob.	Deſcription.
70	Mar. 15	30 (ρ) Virg	f	0 45	n	1 7	1	A nebula.
71	–	– –	f	2 0	n	1 0	1	S.
72	–	34 Virginis	f	0 15	ſ	0 54	1	S. lE.
73	–	– –	f	4 0	ſ	1 3	1	F. not vS.
74 75	–	– –	f	5 30	ſ	0 42	1	Two, nearly par. The p. pB nearly R. The f. pB. vmE. 8 or 10′ diſtance.
76	–	20 (χ) Serp^{s}	p	3 12	ſ	0 42	2	pB. pL. lE. gbM. r.
77	19	52 (K) Leonis	p	4 42	ſ	0 27	2	pB. pL. E. b. M. r. f. pBſt.
78	–	– –	p	0 12	ſ	0 27	2	pB. pL. r.
79	–	15 Bootis	f	18 30	ſ	0 15	1	F. L. R. lbM. r. 4 or 5′ dia^{r}
80	21	47 (δ) Cancri	f	4 45	n	0 55	1	pB. pL. E. r. 2 or 3 ſt in it.
81	–	51 (*m*) Leon	f	1 15	ſ	1 41	1	pB. pL. not R. r.
82	–	– –	f	8 15	ſ	1 35	1	F. S. lE. r. ſ. pBſt.
83	–	3 Comæ	p	0 15	ſ	0 43	1	F. pL. r.
84	–	– –	f	12 30	ſ	0 59	1	F. S. R. r. Note.
85 86	–	25 Comæ	p	13 0	ſ	0 21	1	Two. The p. pB. S. The f. F. S.
87	–	– –	p	11 15	ſ	1 33	1	S. bM. r.
88	–	– –	p	10 45	ſ	0 53	1	S. bM. r.
89	–	6 Comæ	f	11 12	n	0 31	2	S. bM. r. near Bſt.
90	–	25 Comæ	p	7 30	ſ	0 3	1	pB. bM. r. near Bſt.
91	–	– –	p	6 0	ſ	0 18	1	vS.
92	–	– –	p	5 30	ſ	0 24	1	S.
93	–	– –	p	4 0	ſ	0 51	1	F. vS.
94	–	– –	p	2 0	ſ	1 35	1	F. S.
95	–	27 Comæ	f	2 45	ſ	1 23	1	pB. vmE. nearly mer.
96	–	15 Serpentis	f	1 0	ſ	1 5	1	pB. pL. not R. bM. r.
97	–	26 Serpentis	f	2 30	n	0 35	1	pF. S. r. p. 2 pBſt.
98	23	8 Leonis	f	15 45	n	0 20	2	F. cL. iR. mbM. 4 or 5′ dia^{r}
99	April 8	52 (K) Leo	f	0 42	ſ	0 12	1	pB. S.
100	–	– –	f	13 30	n	0 42	1	F. pS. r.
101	–	– –	f	14 0	ſ	0 20	1	pB. S. mbM.
102	–	70 (θ) Leonis	f	0 48	ſ	0 38	1	F. pS. R. lbM. r.
103	–	94 (β) Leonis	p	9 12	n	0 47	1	F. S. E. r. 2 or 3 ſt viſible in it.
104	–	– –	p	3 18	ſ	0 48	2	pB. S. R. r. pLrN.
105	–	34 Virginis	p	35 6	n	1 17	2	pB. pL. R. vgmbM. r.
106	–	– –	p	33 18	n	1 28	2	F. pL.
107	–	6 Comæ	p	0 24	ſ	1 0	1	pL.
108	–	– –	p	0 6	ſ	1 2	1	mE. r.
109	–	– –	f	0 24	ſ	1 54	1	r.
110	–	– –	f	3 12	ſ	0 3	1	S. r.
111 112	–	– –	f	5 18	ſ	0 18	1	Two, about 2′ diſtant. The firſt R. r. The 2d, E. r.

II.	1784	Stars.		M.	S.		D.M.		Ob.	Deſcription.
113	April 8	6 Comæ	f	10	54	n	0	9	1	E. r.
114	—	34 Virginis	p	16	54	n	1	28	2	F. r.
115 116	—	6 Comæ	f	14	0	ſ	1	21	1	Two. Both r.
117	—	— —	f	14	24	ſ	0	54	1	r.
118	—	— —	f	15	36	ſ	0	28	1	S. Note.
119	—	— —	f	18	54	n	0	35	1	pL. r.
120	—	— —	f	19	6	ſ	0	25	1	L. r.
121 122	12	34 Virginis	p	17	54	n	1	21	1	Two. Both pF. S. bM.
123 124	—	— —	p	16	24	n	0	29	2	The two p. of 3. Both F. S. bM. Note.
125	—	— —	p	4	18	n	1	25	1	not vF. S. r.
126	—	— —	p	3	0	n	1	11	2	pB. L. E. r.
127	—	— —	p	2	30	n	1	39	1	F. vS. R. lbM. r.
128	—	— —	f	0	42	n	1	54	1	L. R. bM. r.
129	—	41 Virginis	f	19	0	n	0	11	1	F. pL. lbM. R. r.
130	—	20 (χ) Serp^s	p	12	30	n	0	12	1	F not S. iF. r.
131	13	56 Leonis	p	3	48	ſ	0	26	1	pB. vL. nearly R. lbM.
132	—	8 (π) Virgin	p	4	48	n	0	7	1	pL. E. pBM. r.
133	—	) (o) Virginis	f	7	46	ſ	1	41	2	not vF. S. E. mer.
134	—	11 (s) Virgin	f	4	24	ſ	0	0	1	F. mE.
135	—	— —	f	5	54	n	0	38	1	S. E. pBM.
136	—	— —	f	6	30	n	1	39	1	F. S. iF. r.
137	—	9 (o) Virginis	f	12	32	ſ	2	2	2	F. pL. r.
138	—	11 (s) Virgin	f	9	6	n	0	20	3	F.
139 140	—	— —	f	9	30	n	0	0	3	Two. The 1ſt is the largeſt. The 2d vF.
141 142 143	—	— —	f	13	18	n	0	18	3	Three nebulæ. The laſt is the largeſt.
144	—	31 (1 d) Virg	p	17	9	n	0	42	2	F. pL. the largeſt of 2.
145	—	60 (σ) Virg	p	50	28	n	0	30	2	vF. S. E.
146	—	31 (1 d) Virg	p	14	6	ſ	0	34	1	F. pL.
147	—	— —	p	7	36	ſ	0	20	1	pB. pL. mE. r.
148	—	— —	p	0	18	n	0	31	1	not F. R. vgbM.
149	—	— —	f	0	9	n	0	53	4	F. pL. iF. r.
150	—	24 (α) Serp^s	p	73	42	n	1	10	1	F. pL. nearly R. er.
151	—	12 Herculis	f	4	18	ſ	0	26	1	not vF. pL. iR. bM. r.
152	15	78 (ι) Leonis	f	4	24	ſ	1	1	1	F. mE. r.
153 154	—	2 (1ξ) Virgin	p	3	0	n	2	2	1	Two, about 5′ diſtant. Both F. pS. C.
155	—	20 Virginis	p	6	6	ſ	0	40	1	F. pL. lE. lb. towards p. ſide.
156	—	— —	p	4	36	ſ	0	26	1	F. pL. lE. r.

II.

II.	1784	Stars.		M.	S.		D.M.		Ob.	Deſcription.
157	April 15	20 Virginis	p	3	36	ſ	1	29	1	F. pL. mE. bM. r.
158	—	31 (1 *d*) Virg	p	8	38	n	1	51	3	pT. pL. nearly R. r.
159	17	81 Leonis	f	0	36	n	0	24	1	pB. S. bM. almoſt ſtellar.
160	—	— —	f	1	0	n	0	45	1	cL. R. vgbM.
161	—	90 Leonis	f	5	36	n	0	53	1	F. not S. R. bM.
162	—	34 Virginis	p	51	54	n	0	0	2	not vF. pL. iR. lb. towards f. ſide.
163	—	— —	p	33	6	n	1	13	1	pS.
164	—	— —	p	32	48	n	0	13	1	pS. vmE.
165	—	— —	p	32	30	n	1	13	1	F. vmE.
166	—	— —	p	27	36	n	0	53	1	pB. vS.
167	—	— —	p	21	30	n	0	49	1	Two nebulæ.
168										The moſt ſ. E.
169	—	— —	p	20	30	n	0	40	1	S.
170	—	— —	p	19	42	n	0	49	1	F.
171	—	— —	p	19	6	n	0	20	1	Three nebulæ.
172										The two firſt vS.
173										The third S.
174	—	— —	p	17	48	n	1	16	1	F.
175	—	— —	p	12	36	n	1	9	1	pF. L.
176	—	— —	p	3	48	ſ	0	37	1	F.
177	—	20 Bootis	f	3	30	ſ	1	42	1	pF. not S. lbM. r.
178	—	28 (β) Serp'	p	12	6	ſ	0	7	2	Two, very cloſe. Both S. ſtellar. The ſ. is largeſt.
179										
180	22	15 (η) Virg	f	8	59	ſ	1	18	3	pB. L. iR. er.
181	—	29 (γ) Virg	f	5	18	ſ	0	58	1	pF. pL. E. r.
182	—	— —	f	6	24	ſ	1	54	1	pF. pL. E. r.
183	24	51 (θ) Virg	p	30	36	n	0	14	1	pB. cL. E. vſmbM.
184	—	— —	p	28	30	n	0	26	1	not F. L. lE. lbM. r.
185	—	— —	p	11	0	n	0	10	1	F. S. iF. near pBſt.
186	25	28 Virginis	f	12	6	n	0	51	1	pF. cL. R. r.
187	—	— —	f	12	42	n	0	37	1	pF. pL. r.
188	—	— —	f	22	54	n	0	57	1	F. cL. E. r.
189	—	72 (1 *l*) Virg	p	21	54	ſ	0	18	1	pB. R. vſmbM. near Bſt.
190	—	26 (χ) Virg	f	23	44	ſ	0	6	2	F. pL. iR. lbM. r.
191	May 9	49 (*g*) Virg	p	4	6	ſ	0	46	1	pF. pS. R. r. near ſome Sſt.
192	—	18 Libræ	f	10	36	ſ	0	16	2	pF. pL. lE. mer. nearly.
193	11	100 (λ) Virg	p	59	30	n	0	48	2	The moſt n. of 3. pB. vS. bM.
194	19	12 (*d*) Bootis	f	7	42	ſ	0	2	2	F. pL. R. mbM.
195	21	39 Ophiuchi	p	12	54	n	1	42	2	pB. cL. iR. lbM. r.
196	22	54 Hydræ	p	6	42	ſ	1	2	1	pB. S. nearly R. bM. r.
197	—	51 (ε) Ophiu	f	35	36	ſ	1	13	1	pB. pL. iR. r.
198	24	3 (*p*) Sagitt'	f	18	42	ſ	0	4	1	pF. not L. crookedly E. er.
199	June 16	64 (ν) Ophiu	f	2	48	n	0	48	1	pB. pL. R. gbM. r.
200	24	10 (γ) Sagitt'	p	1	6	n	0	22	1	F. pS. r. unequally B.

II.	1784	Stars.		M.	S.		D.M.	Ob.	Deſcription.
201	July 13	18 Sagittarii	p	7	54	ſ	0 55	1	F. pL. lbM. r.
202	17	12 (φ) Cygni	f	17	36	ſ	0 53	1	A reſolvable nebulous patch of ſt.
203	—	65 (ζ) Cygni	p	9	30	ſ	0 16	2	pB. pL. iE. bM.
204	Aug. 7	24 Sagittarii	p	9	18	n	0 50	1	pB. S. ſtellar. not verified.
205	—	— —	p	1	42	n	0 33	1	pB. cL. iE. bM.
206	Sept. 7	52 (k) Cygni	f	5	36	n	1 22	1	F. S. crookedly E. r.
207	—	44 (η) Pegaſi	p	34	27	n	1 15	1	cL. R. gmbM. er.
208	10	84 (ψ) Pegaſi	p	13	48	n	1 0	1	F. cL. R. vgbM. ſf. ſt.
209	—	34 (ζ) Andr	p	5	57	n	1 12	2	F. pL. iR. equally B. r.
210	11	31 (δ) Andr	f	18	12	ſ	0 26	1	F. pL. unequally B. near pBſt.
211	—	13 Triang	f	5	24		0 35	1	F. pL. lE. bM. n. 2 ſt.
212	12	63 Pegaſi	p	19	42	ſ	0 15	1	pB. pL. lE. mbM. r. ſ. 2 Fſt.
213	—	79 Pegaſi	p	2	36	n	0 42	1	F. pL ER. lbM.
214	—	40 Androm	p	7	18	ſ	0 15	1	F. E. p. Bſt.
215	—	— —	f	4	30	n	0 41	1	Three. mer. Nearly equal in ſize. All. F. vS. R. proportion of diſt. ſ to n. 2 to 1.
216									
217									
218	—	— —	f	5	30	n	1 22	1	F.
219	—	— —	f	7	36	n	1 22	1	Two. The p. F. vS. The f. pL.
220									
221	—	3 (ι) Triang	p	6	12	ſ	0 15	1	F. pL. mE. r. 1½′ l.
222	—	— —	p	5	12	ſ	10 0	1	F. pL. mE. r. 1½′ l.
223	—	— —	p	2	12	ſ	1 52	1	pB. pS R.
224	13	43 (β) Andr	p	0	18	n	0 5	1	pB*. cL. R. bM. * Though β And. in the field.
225	—	9 (γ) Trian	f	4	18	ſ	0 39	1	F. vS. R.
226	15	71 (y) Pegaſi	p	4	54	ſ	0 5	1	F. pL. bM. elliptical.
227	—	89 (χ) Pegaſi	p	10	18	n	0 32	2	F. cL. mE. r.
228	—	6 (β) Arietis	p	5	12	n	1 7	1	Two. Both F. pS. iR.
229									
230	18	81 (φ) Pegaſi	p	1	27	n	1 4	1	F. pL. R. bM. r.
231	—	— —	p	1	3	n	0 59	1	F. pL E. par. contains a ſtell. or ſt.
232	—	— —	f	6	45	n	1 35	1	F. S. R. or large ſtellar.
233	19	47 (λ) Pegaſi	p	9	3	n	0 12	3	Two. The p. pB. lE. nearly mer.
234			p	8	33	n	0 14	3	The f. F. E. nearly par. 1′½ l.
235	20	11 Piſcium	p	13	43	ſ	0 36	2	F. pL. broadly E.
236	—	90 (φ) Aqua	f	3	53	n	1 22	4	pB. pL. iR. mbM.
237	—	79 Ceti	p	4	48	n	0 36	1	F. E. mer. 2′ l.
238	Oct. 6	26 (β) Perſei	p	28	34	ſ	0 10	2	pB. mE. near par. mbM. 4′ l 1′ b.
239	7	27 (κ) Perſei	p	8	27	n	0 2	1	The 1ſt of 2. pB. pS. r.
240	8							1	pF. pL. iR. er.
241	—							1	pS. C.
242	11	48 (μ) Pegaſi	p	39	50	ſ	0 54	2	F. S. iR. near and p. 2 or 3 ſt.
243	—	— —	f	6	27	ſ	0 54	2	F. S. iR.

II.	1784	Stars.		M.	S		D.M.		Ob.	Deſcription.
244	Oct. 14	54 (α) Pegaſi	f	30	48	n	0	6	2	F. S. lE.
245	—	58 Piſcium	p	3	36	n	2	16	4	pB. pL. R. lbM.
246	—	19 Arietis	f	4	54	ſ	0	49	1	F. pL. E. 4 or 5′ ſ. cſt.
247	15	13 Pegaſi	f	10	0	n	0	28	1	pB. R. bM. 1′ d.
248	—	54 (α) Pegaſi	p	38	0	n	0	59	2	F. pS. a quartile with 3 Sſt.
249	—	– –	p	3	36	n	1	11	2	F. pS. E. f. pBſt.
250	—	47 Piſcium	p	67	12	ſ	0	37	1	F. lE. p. vBſt.
251	16	54 (α) Pegaſi	p	4	36	n	0	44	1	pB. cL. E. r.
252	—	102 (π) Piſc	p	12	48	n	0	45	1	F. pL. oval. lbM. p. pBſt.
253	—	– –	f	5	54	n	1	30	1	pB. pL. E. bM. r.
254	—	38 Arietis	f	8	48	n	0	34	1	F. S. iR. r.
255	18	82 Pegaſi	p	8	21	ſ	0	11	2	pB. pS. R. gbM. r.
256	—	77 Pegaſi	f	1	0	ſ	0	25	1	F. R. gbM.
257	—	34 Piſcium	f	12	6	ſ	0	39	2	F. pL. iR. mbM.
258	20	15 Eridani	p	8	54	n	1	54	3	F. vL. lbM. R. 7 or 8′ d.
259	Nov. 16	43 (γ) Cancri	p	20	58	n	1	2	1	F. S. iF.
260	—	4 (λ) Leonis	f	3	22	ſ	1	16	1	F. pS. lE.
261	17	12 Pegaſi	f	2	8	ſ	0	46	1	F. iR. leſs than 1′ d.
262	—	27 Eridani	p	11	51	ſ	1	40	1	F. l and iE. above 1′ d.
263	—	– –	p	9	28	ſ	1	15	1	not vF. bM. 1′½ d.
264	—	47 (δ) Cancri	p	67	42	n	2	20	1	F. S.
265	20	4 (1 χ) Can	p	19	20	n	1	28	1	pF. pS. iF. lE. bM.
266	—	15 (ι) Nav	f	25	33	n	1	25	1	F. E. bM. r. 1′½ d.
267	Dec. 9	27 Eridani	p	6	1	n	0	40	1	F. vS. R. lbM.
268	—	8 (ι) Crateris	p	63	16	ſ	0	16	1	F. S. R. SB point M. C.
269	—	10 Crateris	f	4	26	n	1	22	1	pB. pL. lE mbM.
270	13	106 (ν) Piſc	f	11	56	ſ	1	11	1	pB. S. iR. mbM.
271 272	—	– –	f	14	54	n	0	11	3	Two, very cloſe. nearly par. The f. ſmalleſt and moſt n.
273	—	86 (γ) Ceti	p	0	14	n	1	44	1	F. S. iR.
274	—	92 (α) Ceti	p	3	54	ſ	0	47	1	F. vS. iE. er.
275	20	32 (2 τ) Hyd	f	9	55	n	1	32	2	pB. cL. iR.
276	—	10 (r) Virgin	p	6	58	n	0	5	3	F. pL. R. lbM.
277	—	– –	p	5	14	ſ	0	1	3	F. S.
	1785									
278	Jan. 6	75 Ceti	p	1	38	ſ	0	5	1	pB. S E.
279	—	35 Eridani	f	2	55	ſ	0	38	2	F. mE. vlbM. about 4′ l.
280	—	14 Hydræ	f	5	2	n	0	21	1	F. vS. lE. ver. 240.
281	—	28 (A) Hydr	p	29	27	n	1	40	2	F. vS. E.
282	10	41 Ceti	f	17	28	n	0	20	2	pB. cL. lE. mbM.
283	—	– –	f	21	26	n	0	10	2	pB. S. mbM.
284	—	80 Ceti	f	3	34	ſ	0	19	1	F. mE. about 3′ l and ¾′ b.
285	—	55 (ζ) Ceti	f	74	50	n	1	2	2	pB. E. ſp nf. about 1′½ l.
286	—	– –	p	4	34	ſ	0	9	1	F. pL. R. lbM. ſ. Sſt.

II.

II.	1785	Stars.		M.	S.		D.	M.	Ob.	Deſcription.
287	Jan. 27	17 Eridani	p	10	24	ſ	1	12	2	F. vS. lE. er. unequally B.
288	28	21 Eridani	p	1	55	n	0	35	3	F. pL. iR. r.
289	31	7 (ν) Lepor	f	2	32	n	0	51	1	F. pL. i triangular F. r.
290	Feb. 1	89 (π) Ceti	f	49	17	n	0	21	3	F. pL. R. lbM. ſ. pLſt.
291	—	26 (π) Erid	p	3	39	ſ	1	25	1	pF. mE. mer. 3 or 4' l and 1' b.
292	4	5 (μ) Lepor	p	0	50	n	0	29	1	pB. iR. mbM. ſp. pcſt.
293	7	6 (3 *b*) Crater	p	52	51	n	0	23	1	pB. S. iR. bM.
294	—	31 Crateris	p	6	45	n	0	6	1	F. S. E. r.
295	—	– –	p	1	48	n	1	18	1	F. vS. iF. bM.
296	—	– –	p	0	12	n	0	24	1	pB. pL.
297	—	89 Virginis	p	11	47	n	0	18	1	pF. L. mbM.
298	8	8 (η) Corvi	f	18	44	n	1	51	1	F. pL. lbM. ¼' p. is a S ſuſpected ſtellar.
299	—	53 Virginis	p	12	30	n	0	48	1	pB. pL. mbM.
300	—	– –	p	11	0	n	2	8	2	pF. eL.
301	—	– –	p	3	8	n	0	34	1	pB. pL. iR. mbM.
302	28	2 (1 ω) Cancri	p	3	5	ſ	1	40	1	pF. vS. bM. er.
303	—	19 (λ) Cancri	p	2	22	ſ	0	35	1	F. S. mbM. r.
304	Mar. 4	11 Monoc	f	30	53	ſ	0	37	3	Some Sſt with pB nebuloſity.
305	5	20 Sextantis	p	7	14	n	0	49	1	F. S. lE. er.
306	—	88 Virginis	f	0	52	ſ	0	24	1	F. vS. iF. r.
307	—	– –	f	3	58	n	0	43	1	F. cL. iF. bM.
308	—	82 (*m*) Virg	f	12	28	n	1	6	2	F. S. iR. lbM.
309 310	—	99 (ι) Virg	p	12	31	ſ	0	1	1	Two. nearly mer. diſt. 4' Sſt. betw. che. touch. n. pB. cL. mbM. ſ. F. S.
311	10	6 (3 *b*) Crate	p	68	34	ſ	1	18	2	cB. S. mbM.
312	—	45 (ψ) Hydr	f	9	41	n	2	0	1	F. L. iR. vgbM.
313	—	– –	f	10	53	n	1	16	1	pB. lE. par. b towards f. ſide.
314	—	– –	f	17	57	n	1	55	1	F. S. iF. bM.
315	11	23 (2 φ) Can	f	0	29	ſ	1	0	2	F. S. R. bM. C. N.
316 317	12	64 (1 *b*) Gem	p	4	16	n	1	17	1	Two. ſp nf. diſt. 1' che. mix. Both F. S. equal. N.
318	—	22 (1 φ) Can	f	8	38	n	0	36	1	F. pL. lE. mbM. r.
319	—	48 (1 ι) Canc	p	9	10	ſ	0	5	1	F. S. bM. r.
320	13	23 Leonis min	p	12	38	n	1	50	1	F. pS. R. lbM.
321	—	13 Can. ven.	p	51	31	ſ	0	50	1	pB. L. gbM.
322 323	—	– –	p	40	19	ſ	1	28	1	The two firſt of 3 in a line. of unequal ſize and brightneſs.
324	—	– –	p	38	3	n	0	17	1	F. S.
325	—	– –	p	26	51	ſ	0	30	1	F. pL. E. bM.
326	—	– –	p	14	11	ſ	0	4	1	F. mE. mer.
327	—	– –	f	19	43	ſ	0	35	1	F. pS.
328	—	– –	f	23	43	n	0	38	1	pB. pS. nearly R. mbM.

II.	1785	Stars.		M.	S.		D.	M.	Ob.	Deſcription.
329	Mar. 13	49 (δ) Bootis	p	48	50	n	0	5	3	pF. S. R. r. n. 2 pBſt.
330	—	— —	p	45	45	ſ	2	2	1	pB. pL. R. bM.
331	16	11 Urſæ min	p	60	36	ſ	0	2	1	F. pS er.
332	—	— —	p	20	10	ſ	0	2	1	pB. cL. b towards p. ſide.
333 334	April 3	27 Urſæ	f	20	14	ſ	0	2	1	Two. Nearly mer. Moſt n. pB. pS. bM. Moſt ſ. F. S. bM.
335	—	— —	f	73	0	n	1	41	1	pF. cL. iE.
336	—	— —	f	88	16	n	0	30	1	pB. vS. iR.
337	—	— —	f	94	42	n	0	48	1	pF. pS. bM.
338	6	44 Leonis min	f	31	8	ſ	0	59	2	F. cL. iR. gvlbM.
339	—	53 Leonis min	f	19	26	n	1	0	1	pF. pS. iF.
340	—	72 Leonis	f	25	8	n	1	35	2	F. vS. ſtellar. ſhort ray p. ſide.
341	—	4 Comæ	p	29	46	n	0	35	1	F. ſtellar.
342	—	— —	p	22	2	n	0	37	1	F. pL.
343	—	21 (*g*) Comæ	f	4	36	n	1	56	1	not L.
344	—	— —	f	20	56	n	1	11	1	F. pL. lE.
345	—	— —	f	23	34	n	2	28	1	juſt ſ. pBſt.
346	—	31 Comæ	f	4	54	ſ	0	34	2	F. pL. iF.
347	10	36 (ζ) Leonis	f	11	14	ſ	0	33	1	pB. S. bM. r.
348	—	41 Leonis min	p	3	34	n	0	51	1	F. S. lE.
349	—	72 Leonis	f	14	12	n	1	1	1	F. pL. i triangular F.
350	—	— —	f	16	2	ſ	0	17	1	F. S.
351	—	— —	f	18	2	n	1	22	1	F. S.
352	—	92 Leonis	p	3	9	n	1	16	2	F. pS.
353	—	7 (*b*) Comæ	p	4	37	n	0	11	1	pB. cL. iF. bM.
354	—	— —	p	0	43	n	0	2	1	F. vS.
355	—	22 Comæ	p	4	32	ſ	1	28	1	pF. L. broadly E.
356	—	40 Comæ	f	5	38	n	1	25	1	pB. S.
357	—	12 (*d*) Bootis	f	18	46	ſ	1	53	1	F. S. iF. lbM.
358	11	39 Leonis mi	p	8	4	n	0	8	1	F. pL.
359	—	— —	p	7	38	n	1	1	1	pB. pS. nearly R. bM.
360	—	44 Leonis mi	p	1	46	n	0	39	1	F. pL. iF.
361	—	— —	f	0	50	n	0	30	1	F.
362	—	— —	f	1	18	n	0	1	1	pB. pL.
363	—	— —	f	1	32	n	0	8	1	F. S.
364	—	— —	f	4	35	ſ	0	44	1	pF. pL. lE. b towards ſf. ſide.
365	—	— —	f	14	2	n	0	56	1	F. mE. 1′½ l. but v. narrow.
366	—	— —	f	14	24	n	0	4	1	pF. pL.
367	—	— —	f	42	12	n	0	14	1	F. vS.
368	—	14 (*b*) Comæ	p	28	42	n	0	59	1	pF. bM.
369	—	— —	p	27	58	n	0	12	1	F. pL. E. b towards f. ſide.
370	—	— —	p	20	16	n	0	55	1	pB. cL. mb towards nf. ſide.
371	—	— —	p	17	40	n	1	55	1	One of three. F. iF.

II.

II.	1785	Stars.		M.	S.		D.	M.	Ob.	Deſcription.
372	Apr. 11	14 (*b*) Comæ	p	74	24	n	1	55	1	One of 4. The moſt n. of the p. ſide of a quartile. F. S.
373	—	– –	p	13	28	n	1	16	1	F. L. bM.
374	—	– –	p	12	22	n	1	12	1	F. S.
375	—	– –	p	11	4	n	1	14	1	F. pS.
376	—	– –	p	6	38	n	0	22	1	pF. S. almoſt R. bM.
377	—	– –	p	6	30	n	1	57	1	About 6′ ſf I. 90. pB. S. the place is that of the np.
378	—	– –	p	4	10	n	1	57	1	F. cL. lE.
379	—	– –	p	1	36	n	1	18	1	F. S.
380	—	15 (*c*) Comæ	f	9	8	f	1	22	1	F. pL.
381	—	31 Comæ	p	3	46	f	0	20	1	F. S.
382	—	– –	f	3	16	f	0	9	1	F. pS.
383	—	– –	f	4	26	n	0	12	1	F. pL.
384	—	– –	f	5	2	f	0	23	1	F. pL.
385	—	– –	f	5	40	n	0	4	1	F. pL.
386	—	– –	f	5	54	f	0	6	1	F. pL.
387	—	– –	f	5	48	n	0	55	1	F. pL.
388 389	—	41 Comæ	p	7	46	n	0	22	1	Two. The time taken between them.
390	—	– –	p	7	10	f	0	43	1	F.
391	—	– –	p	7	18	n	0	23	1	F.
392 393 394	—	– –	p	5	46	n	0	14	1	Three. The 2 f. p near each other. The ſp. about 8′ diſt. The time is that of the 2.
395	—	– –	p	3	26	n	0	33	1	F. S.
396	—	– –	p	2	16	n	1	29	1	F. S.
397	—	– –	p	2	2	f	0	4	1	F. S.
398	—	– –	p	1	30	n	0	8	1	F. S.
399	—	3 (β) Coron	f	6	54	f	0	27	1	pF. pL. iR. bM. r.
400	13	26 Bootis	f	47	12	f	1	33	1	F. pL. er.
401	14	11 Serpentis	p	2	14	f	1	35	1	pF. pL. vlbM. r. p. 3 Sſt.
402	—	12 Ophiuchi	p	14	32	n	0	4	1	F. cL. E. ſp nf. r. 3′ l 2′ b.

Third claſs. Very faint nebulæ.

III.	1783	Stars.		M.	S.		D.	M.	Ob.	Deſcription.
1	Nov. 3	36 (υ) Orion	f	3	39	n	1	57	2	vF. S. mE. In the L. neb.
2	Dec. 21	60 Ceti	f	13	::	n			1	eF. vS. R. lbM.
3	30	95 (ο) Leonis	f	4	15	n	0	36	2	vF. vS. lE. r.
	1784									
4	Jan. 18	6 (*h*) Leonis	f	6	4	f	0	9	3	eF. vS. iE. ſp. a triangle of Bſt.
5	—	47 (ρ) Leonis	f	10	0	n	0	9	1	eF. eS. viewed alſo with 240.
6	—	59 (*e*) Virgin	p	28	::				1	vS.

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III.	1784	Stars.		M.	S.		D.M.	Ob.	Deſcription.
7	Jan. 23	3 (β) Can. mi	f	36	30	n	0 19	1	Stellar. 240 left ſome doubt.
8	—	3 Leonis	f	1	6	ſ	0 28	3	E. er. 3 of the ſt. viſible.
9 10	—	32 (2 *d*) Virg	f	46	54	ſ	0 25	2	Two. Both vF. and vS.
11	—	31 Bootis	p	38	15	ſ	0 1	1	vF. ſtellar.
12	—	— —	p	21	15	ſ	0 34	1	vF. forming an arch with 3 ſt.
13	28	11 (*s*) Virg	f	27	30			1	eF. not verified.
14	30	31 Bootis	p	12	30	ſ	0 9	1	eF. vL. not verified.
15 16	Feb. 15	68 (δ) Leonis	f	7	30	ſ	0 24	1	Two. The p. vF. L. 5 or 6′ dia. The f. eF. S. Fig. 5.
17	23	16 (*c*) Virgin	f	6	0	ſ	0 47	1	vF. pS. r.
18	—	— —	f	11	45	n	0 38	1	vF. cL. r.
19	Mar. 11	2 (ε) Can. mi	f	5	16	n	0 28	1	2vS and cloſe ſt. with nebuloſity left doubtful.
20	—	53 (*l*) Leonis	f	1	45	ſ	0 26	1	vF. r.
21	—	73 (*n*) Leonis	p	15	36	ſ	1 11	2	vF. S. C. ver. 240.
22	—	53 (*l*) Leonis	f	14	0	n	0 31	1	vF. vS. with 240 cL.
23	—	73 (*n*) Leonis	p	9	6	ſ	1 56	2	vF. vS. lE. ver. 240.
24	12	20 Leonis	f	11	30	n	1 19	1	vS. 240 left ſome doubt.
25	—	— —	f	26	15	n	0 0	1	vF. S.
26	—	20 Comæ	f	4	30	ſ	0 37	1	eF. L. left doubtful.
27	14	86 Leonis	p	13	30	ſ	0 22	1	The moſt ſ. of 3.vF.vS.II.50.51.
28	—	— —	p	2	30	ſ	1 10	1	vF. L. r.
29	—	— —	f	7	15	ſ	0 34	1	vF. eS. ſtellar. ver. 240.
30	—	— —	f	10	0	ſ	0 40	1	vF. pS. f. 2 vBſt.
31	—	11 Comæ	f	3	15	ſ	0 10	1	eF. forms a triangle with 2 Sſt.
32	—	8 (η) Bootis	f	16	0	ſ	0 56	1	vS. or nebulous double ſt. ver. 240.
33	—	5 (*r*) Herculis	f	3	30	n	1 30	1	eF. pL. partly ver. 240.
34	15	5 (ξ) Leonis	f	13	0	ſ	0 10	1	eF. vS. completely ver. 240.
35 36	—	78 (ι) Leonis	f	20	30	ſ	0 15	1	Two. par. 3 or 4′ diſt. Both eF. vS.
37	—	12 (*t*) Virgin	p	8	15	n	0 40	1	eF. vS. with 240. cL.
38	—	— —	f	11	15	n	0 20	1	vF. vS.
39	—	— —	f	12	45	n	0 15	1	vF. near ſome Bſt.
40	—	30 (ρ) Virg	p	11	15	n	0 31	1	eF. pL. eaſily overlooked.
41	—	— —	p	10	45	n	1 14	1	vF.
42	—	— —	p	10	15	n	1 23	1	vF.
43	—	34 Virginis	p	6	6	ſ	0 4	2	vF. pL. lE. contains two ſt.
44	—	— —	p	3	30	ſ	0 25	2	The p. of 2. vF. S. Note.
45 46	—	71 Virginis	f	0	37	n	0 12	1	Two. miſtaken for one; but 240 ſhewed them both. cL. vF.
47	—	32 Bootis	p	25	0	ſ	0 47	1	vF. r. 2 or 3 ſt in it.
48	—	— —	f	3	30	n	0 27	1	eF.
49	19	62 (1 ο) Can.	p	14	33	ſ	1 5	2	F. cS. lE. np ſf. like 2 joined.

III.	1784	Stars.		M.	S.		D.	M.	Ob.	Deſcription.
50	Mar. 19	45 (1A) Can	f	3	15	f	0	4	1	eF. ver. 240. and cL. R.
51 52	—	27 (ν) Leonis	p	7	0	n	0	21	1	Two. np. ff. 6 or 7′ diſt. Both eF. p is the largeſt.
53	—	34 Leonis	f	1	0	f	0	41	2	eF. S. lE. r. 3 or 4 ſt in it.
54	—	52 (K) Leon	p	10	45	f	1	27	1	eF. cL. R. r. no N.
55	—	46 (i) Leoni	f	4	18	n	0	3	2	vF. vS. iR. r. ſome ſt. in it.
56	—	15 Bootis	p	13	0	f	0	40	1	eF. vS. E. r.
57	—	– –	p	10	30	f	0	28	1	eF. S. ver 240.
58	—	– –	p	8	30	f	0	43	1	eF. S. ver 240 and lE.
59	—	– –	p	6	15	f	1	10	1	eF. S. ver 240.
60	21	47 (δ) Cancri	f	20	0	n	0	23	1	vF. S. with 240 near Sſt.
61	—	– –	f	26	30	f	0	18	1	eF. 240 ſhewed 5 Sſt with nebuloſ.
62 63	—	– –	f	31	30	n	0	50	1	Two. nearly mer. Both vF. pS. R. lbM. r. with 240 cL.
64	—	– –	f	36	0	n	0	52	1	eF. 240 ſhewed ſome Sſt with neb.
65	—	51 (m) Leon	p	38	15	f	0	33	1	vS. E. r. better with 240
66	—	– –	p	9	15	f	0	44	1	vF. S. E. r. the ſame with 240
67	—	– –	f	11	45	f	1	45	1	vF. nebul. betw. 2 ſt. 2′ l. ver. 240
68	—	3 Comæ	p	1	45	f	0	40	1	2 vSſt with ſuſp. neb. 240 doubtf.
69	—	25 Comæ	p	5	0	f	0	18	1	vF. S.
70	—	27 Comæ	f	6	0	f	0	42	1	vF. not S.
71	—	42 Comæ	f	19	30	f	0	41	1	3 Sſt with ſuſpect. nebul. 240 left ſome doubt.
72	—	4 (τ) Bootis	p	10	15	f	1	26	1	eF. vS. ver 240 and cL.
73	—	5 (r) Herc	p	4	0	f	1	50	1	eF. vS. eaſily ver. 240.
74	—	48 Serpentis	p	1	15	n	0	5	1	vF. S. ver. 240
75	Apr. 8	70 (θ) Leonis	p	12	24	f	1	7	1	eF. not S.
76	—	– –	f	4	0	f	0	41	1	eF. pL. eaſily ver. 240.
77	—	94 (β) Leonis	f	12	12	f	1	12	1	eF. pL. R. r.
78	—	6 Comæ	f	17	18	f	0	19	1	vF. r. by moon-light.
79	12	73 (n) Leonis	p	5	6	f	1	25	1	eF. not L. lE. r.
80	—	– –	f	18	36	f	0	48	1	vF. vS. R. bM. ſtellar. ver. 240
81	—	– –	f	22	35	f	1	11	2	vF. vS. R. ſtellar.
82	—	41 Virginis	p	1	42	n	1	7	1	vF. S. E. r.
83	—	– –	f	6	18	n	0	0	1	vF. S. iF. r.
84	—	70 Virginis	p	3	42	f	0	4	1	eF. vS. ſtellar. ver. 240.
85			f	6	12	n	0	1		Three. The two p. vF. S. R. The laſt vF. pL. R. Place of the 2d not taken.
86	—	– –	..	..	..	..	..	..	1	
87			f	6	48	f	0	9		
88	13	56 Leonis	p	5	42	f	0	23	1	eF. no time to ver.
89	—	63 (χ) Leon	f	6	24	f	1	29	1	eF. a little doubtful.
90	—	3 (ν) Virginis	f	4	54	n	0	1	1	vF. vS. vlbM.
91	—	11 (s) Virg	f	7	48	n	1	19	1	The f. of 2. eF. II. 17.

III.	1784	Stars.		M.	S.		D.M.		Ob.	Deſcription.
92 93	April 13	9 (*o*) Virginis	f	16	15	ſ	2	4	2	Two. One vF. vS. The other juſt by. eF. eS. left doubtful.
94 95 96	—	– –	f	18	22	ſ	1	46	2	Three. All. eF. vS. R. In the 2d obſervation two of them were overlooked.
97	—	31 (1 *d*) Virg	p	17	9	n	0	42	2	The ſmalleſt of 2. eF. II. 144.
98	—	– –	p	3	6	n	0	0	1	eF. eS. The place not accurate.
99	—	32 (2 *d*) Virg	f	47	36	ſ	0	33	1	eF. S.
100	—	– –	f	50	42	ſ	1	8	1	eF. E.
101	—	– –	f	51	0	ſ	0	23	1	eF. pL. R. er. The ſt almoſt viſible
102	15	2 (1ξ) Virgin	p	1	46	n	1	54	1	eF. pL.
103	—	– –	f	0	24	n	0	58	1	vF. r.
104	—	4 (2ξ) Virgin	p	2	12	n	0	19	1	vF. vS. left doubtful. Twilight.
105	—	31 (1 *d*) Virg	p	1	52	n	1	35	2	eF. vL. lbM.
106	—	33 Virginis	f	7	30	n	0	8	1	vF. pL. vlbM. r.
107	17	48 Leonis	f	6	54	.	0	8	1	eF. pL. a little doubtful. Twil.
108	—	63 (χ) Leonis	p	13	18	n	1	7	1	eF. eS. r.
109	—	90 Leonis	f	5	18	n	0	49	1	8 or 10′ ſp. II. 161. vS. ſtellar. not ver.
110	—	20 Bootis	f	1	54	ſ	2	29	1	vF. vS. lE. ver. 240.
111	18	58 (*d*) 84 (τ) Leon	f ..	8 ..	36 ..	.. n	.. 1	.. 43	1	vF. vS. r. ver. 240.
112	24	74 (φ) Leonis	f	10	6	ſ	1	52	1	eF. cL. R. r. near vBſt. ☽ light.
113	—	– –	f	34	18	ſ	1	3	1	eF. eS. with 240. 2 vSſt and nebu.
114	25	28 Virginis	p	14	18	n	1	35	1	2 vSſt with nebuloſity with 240 left doubtful.
115	May 9	67 (α) Virg	f	1	12	ſ	1	10	1	vS. vF. ſtellar. ver. 240.
116	—	31 (ι) Libræ	p	8	48	n	0	15	1	vF. cL. nearly R. lm.
117 118	11	100 (λ) Virg	p	59	30	n	0	48	1	The two moſt ſ. of 3. That M. vF. vS. The moſt ſ. eF. eS. ver. 240. II. 193.
119	—	– –	p	55	42	n	0	29	1	eF. vS. ſtellar. ver. 240.
120	—	– –	f	6	24	n	0	9	1	eF. pL. iR. lb towards f. ſide.
121 122	14	9 (α) Libræ	p	27	0	ſ	0	36	1	Two np ſf. The f. eF. 1′ d. nearly R. The p. vF. vS. R. diſt. 5′.
123	15	18 Herculis	f	40	30	ſ	0	47	1	vF. pL. R. lbM.
124	—	– –	f	43	30	ſ	0	47	1	vF. ſtellar. ver. 240.
125	16	25 (ρ) Bootis	p	33	12	ſ	1	10	1	vF. S. iR. lbM. almoſt ſtellar.
126	—	– –	p	2	18	ſ	0	24	1	2 Sſt. with ſuſpected nebul. almoſt ver. 240.
127 128	—	28 (σ) Bootis	f	3	48	n	0	45	1	Two. 3′ diſt. par. The f. vF. vS. iR. The p. eF. vS. ver. 240.

III.	1784	Stars.		M. S		D.M.	Ob.	Deſcription.
129 130	May 16	28 (σ) Bootis	f	17 48	n	0 3	1	Two. about 6′ diſt. Both eF. vS. R. ver. 240.
131	—	— —	f	22 54	n	0 11	1	vF. E. cloſe to a ſt. contains 2 ſt.
132	17	36 (ι) Bootis	p	16 54	n	0 26	1	eF. S. lE. the ſame with 240.
133	—	— —	p	2 36	n	1 35	1	cF. cL. iR. lbM.
134	19	12 (d) Bootis	f	4 28	n	0 12	2	vF. pL. E. par. r.
135	—	— —	f	12 30	n	1 5	1	eF. vS. ſtellar. ver. 240.
136	—	— —	f	14 8	ſ	0 30	2	vF. S. E. nearly par. with 240 like two ſtel.
137	—	76 (λ) Hercu	p	2 54	n	0 22	1	vF. not S. iE.
138 139	2[illegible]	20 (1γ) Libr	f	13 36	n	1 9	3	Two. nearly par. 7′ diſt. Both vF. not vS. R.
140	June 11	27 (β) Hercu	p	23 30	ſ	0 51	1	vF. vS. r. ver. 240. np. pBſt.
141	July 12	16 (ψ) Capri	p	20 42	n	0 33	1	vF. cL. lE. lbM. 240. ſame.
142	21	70 Aquilæ	p	3 39	n	0 31	2	vF. E. about 2′ l.
143	Aug. 7	35 (2ν) Sagit	p	0 0	ſ	0 3	1	3 vSſt with ſuſpected nebuloſity.
144	Sept. 5	39 (b) Cygni	p	21 18	n	1 20	1	Some eSſt. with neb. iE. ver. 240.
145	10	10 (κ) Pegaſi	p	25 48	n	0 53	2	vF. lE. ſtellar.
146	—	59 Pegaſi	f	11 24	n	1 53	1	vF. E. ſome Sſt. with nebuloſity.
147	—	85 Pegaſi	f	7 54	ſ	1 13	1	2 or 3 ſt. with ſeeming nebuloſity.
148	11	28 Androm	p	4 12	ſ	0 32	1	vF. pL. lbM.
149	—	31 (δ) Andr	f	4 24	ſ	0 15	1	eF. vS. R.
150	—	2 (α) Trang	p	18 48	n	1 4	2	Near V. 18. vF. SR. bM.
151	—	6 (ι) Trang	p	7 0	ſ	1 18	1	vF. vS. ſtellar. betw. vL. and Sſt.
152	—	39 Arietis	p	8 12	n	0 49	1	vF. pS. of equal light.
153	12	40 Andr	p	13 0	n	0 29	1	vF. pL. lE. vlb. towards f ſide.
154 155	—	— —	f	9 18	n	0 20	1	Two. Both eF. vS. The f is the largeſt.
156 157 158	—	43 (β) Andr	f	13 6	ſ	2 8	2	Three forming a rect. triangle. In the legs eF. vS. at the rectangle vF. pL.
159 160	—	40 Androm	f	20 6	n	1 30	1	Two. Both eF. S. but unequal.
161	—	17 (r) Perſei	p	14 30	ſ	1 47	2	vF. S. iE. r.
162 163	—	21 Perſei	p	13 42 13 18	n	0 30 0 32	1	Two. Both vF. pS. R. lbM.
164	—	— —	f	15 36	ſ	1 19	1	eF. vS. 240 left a doubt.
165	13	66 (υ) Cygni	f	43 0	n	0 4	1	5 or 6 ſt. forming a parallelogr. with mixed nebul. ver. 240.
166	—	— —	f	78 18	ſ	0 47	1	eF. vS. E. nf. & 4 or 5′ diſt. from I. 53.
167 168	—	43 (β) Andr	f	15 30	ſ	2 12	1	Two. Both ſtellar.
169	—	— —	f	15 12	ſ	1 46	1	ſtellar.
170	—	— —	f	16 30	ſ	1 31	1	ſtellar.

III.	1784	Stars.		M.	S.		D.	M.	Ob.	Deſcription.
171	Sept. 13	43 (β) Andr	f	17	30	ſ	0	56	1	ſtellar.
172 173	—	— —	f	18	0	ſ	2	8	1	Two. Both vS. ſtellar. a little doubtful.
174	—	3 (ι) Triang	p	25	24	n	0	22	1	ſtellar. ver. 240.
175	—	— —	p	12	48	n	2	29	1	ſtellar.
176	—	— —	p	6	6	n	1	0	1	eF. ſtellar. 240 left ſome doubt.
177	—	9 (γ) Triang	f	9	36	ſ	0	17	1	vF. cL. iR. r. 2 or 3′ d.
178	—	17 (γ) Perſei	f	9	6	n	0	13	1	vF. pL. R. SB place M.
179	15	6 (β) Arietis	p	3	0	n	1	30	1	vF. pL. lE.
180	18	40 Pegaſi	p	3	0	n	0	47	1	eF. vS. R. n. cLſt.
181	—	65 Pegaſi	p	6	48	ſ	1	49	1	vF. vS. R. ver. 240.
182	—	40 Pegaſi	f	38	24	ſ	0	51	2	4 or 5 Sſt. with nebul. 240 doubt.
183	—	89 (χ) Pegaſi	f	0	30	ſ	1	38	1	eF. S. iE.
184	20	11 Piſcium	p	17	44	ſ	0	22	2	eF. vS. ſtellar. ver. 240.
185	—	— —	p	12	50	ſ	0	32	2	vF. E. er. 3 Fſt. viſible in it.
186	—	20 Piſcium	p	29	15	ſ	1	41	1	eF. vS.
187	—	— —	p	14	39	n	0	1	1	eF. ſtellar. ver. 240 and cL.
188	—	— —	p	13	33	ſ	0	9	1	eF. ſtellar. juſt like 187.
189	—	— —	p	8	15	ſ	1	52	1	eF.
190	—	29 Piſcium	f	4	54	ſ	0	40	1	vF. vS.
191	—	34 Ceti	p	9	12	ſ	1	53	2	vF. mE.
192	—	72 Ceti	p	17	24	ſ	1	43	1	eF. S. ver. 240. with difficulty.
193	—	— —	p	12	12	ſ	2	6	1	eF. ver. 240. with difficulty.
194	—	81 Ceti	f	38	6	n	0	55	1	eF. eS.
195	—	— —	f	42	42	n	0	49	1	eF. eS. ver. 240.
196 197	—	— —	f	47	0	n	0	36	1	Two. Both eF. ver. 240 but juſt ſuſpected with 157.
198	Oct. 6	12 (q) Perſei	p	3	3	n	0	40	2	cB. mE. vgmbM. near 4′ l.
199	7	27 (κ) Perſei	p	8	27	n	0	2	2	The f of 2. vF. iF. pS. II. 239.
200	14	53 Piſcium	f	4	24	n	1	13	2	2 Sſt with nebuloſity ver. 240.
201	—	19 Arietis	f	4	6	ſ	0	47	1	vF. vS. E. ſ. pcſt.
202	15	47 Piſcium	p	83	54	ſ	1	15	1	eF. vS. ſtellar. ver. 240.
203	—	— —	p	78	18	n	0	18	1	vF. cL. E. 2′ l.
204	—	59 Piſcium	f	0	42	n	0	2	1	vF. S. ſp. 2 vSſt.
205	—	92 Piſcium	p	5	30	ſ	0	10	1	eF. ver. 240. diſcovered in gaging.
206	—	— —	p	3	30	ſ	1	20	1	eF. S.
207	—	8 (ι) Arietis	f	5	12	n	0	32	1	eF. vS. ſtellar. plainly. ver. 240.
208	—	— —	f	6	30	ſ	1	49	1	eF. vS. iR, juſt f. pBſt.
209	16	17 Delphini	f	18	6	ſ	0	11	1	vF. S. R.
210 211	—	54 (α) Pegaſi	p	2	48	n	0	46	1	Two. The p. vF. S. lE. The f. vF. vS. ſtellar.
212	—	— —	f	21	6	ſ	0	59	1	eF. eS. ver. 240. completely though with difficulty.
213	—	— —	f	27	36	n	0	40	1	eF. cL. ver. 240. betw. 2 Bſt.

III.

III.	1784		Stars.		M.	S.		D.M.		Ob	Deſcription.
214	Oct.	16	31 Arietis	p	36	48	n	1	24	1	vF. ſtellar. ver. 240.
215		—	— —	p	36	6	n	0	6	1	eF. ſtellar. diſcovered by 240.
216 }		18	46 (ξ) Pegaſi	f	3	15	ſ	0	37	3	Two. The p. vF. pS R. vlbM.
217 }					3	25		0	32		The f. vF. pS. R. vlbM.
218		—	58 (*n*) Pegaſi	f	13	51	n	0	4	1	eF. pS. lE.
219		19	15 Delphini	p	5	24	n	0	2	1	eF. vS. ſtellar. ver. 240. with dif.
220		—	66 Pegaſi	p	10	10	n	0	23	4	F. R. bM. 1′½ d.
221		—	— —	p	7	10	n	1	0	2	vF. S.
222		—	— —	p	7	7	n	0	54	2	vF. S. R.
223		20	7 (*b*) Ceti	f	23	12	ſ	1	1	1	vF. lE. or oval. 1′ d. np. 2 pBſt.
224		—	1 (1 τ) Erid	p	21	42	ſ	2	11	2	vF. S. iR.
225		—	15 (δ) Lepor	f	6	24	n	0	49	1	eF. E. r. near 1′ l. ver. 240.
226		21	70 (*q*) Pegaſi	p	1	50	ſ	0	18	2	vF. vS. ſtellar ver. 240.
227	Nov.	7	64 Ceti	p	2	24	ſ	0	37	1	2 or 3 Sſt. with neb. nearly ver. 240
228 }		—	73 (2ξ) Ceti	f	12	54	n	0	17	1	Two about 1′ diſt. The p. eF. vS. ver. 240. The f. eF. eS. 240. doubtf.
229 }											
230		12	55 (*l*) Pegaſi	p	3	36	ſ	0	29	1	eF. eS. 240 left ſome doubt.
231 }		—	31 (1 *c*) Piſc	p	9	0	ſ	1	0	1	Two. Both vF. ſtellar.
232 }											
233		—	— —	p	8	27	ſ	1	0	2	eF. pL. glbM.
234		16	43 (γ) Canc	p	11	24	n	1	6	1	vF. ſtellar.
235		—	— —	p	3	20	n	2	4	1	eF. S. ver. 240.
236		—	4 (λ) Leonis	p	23	22	ſ	1	37	1	eF. lE. betw. 2 pBſt. ver. 240.
237		17	33 Pegaſi	f	12	54	n	0	46	1	eF. vS.
238		—	66 Pegaſi	p	6	6	n	1	10	1	eF. eS. ver. 240. with difficulty.
239		—	4 Eridani	p	32	26	ſ	1	1	1	vF. S. 1′ dia. or more.
240		20	12 Leporis	p	7	55	ſ	0	59	1	vF. vS. ſtellar.
241		—	— —	f	3	39	n	0	23	1	eF. vS. lE. par.
242		—	15 (ι) Nav	f	68	16	n	0	53	1	vF. lE. S. 1′ d.
243	Dec.	2	56 Pegaſi	p	9	16	n	0	42	1	vF. S. er.
244		9	48 Ceti	P	48	34	n	0	27	1	eF. vS. E.
245		—	15 Eridani	p	15	49	ſ	0	27	1	vF. cL. iE. r. unequally B.
246		—	19 Eridani	p	1	38	n	0	50	2	vF. E. equally B.
247		—	— —	f	6	5	ſ	1	4	1	eF. vS.
248		—	27 Eridani	p	4	23	n	1	7	1	vF. vS. lE.
249		—	— —	p	2	19	n	1	18	1	vF. vS.
250 }		13	89 (*f*) Piſci	f	2	25	ſ	0	14	1	Two. nearly par. 4 or 5′ diſt. Both vF. vS. R.
251 }					..	..		..	..		
252		—	— —	f	3	42	n	1	38	1	vF. pL. iR. lbM.
253		—	— —	f	6	48	n	0	11	1	eF. cL. E.
254		—	15 Sextantis	p	14	34	n	1	52	2	vF. E. np ſf. 5′ l. ¼′ b.
255		—	7 Sextantis	f	20	27	n	0	42	1	vF. vS. p. triangle of Bſt.
256		20	13 (ζ) Can. mi	f	26	5	ſ	0	48	1	vF. vS. ver. 240.

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III.	1784	Stars.		M.	S		D.M.		Ob.	Deſcription.
257	Dec. 20	13 (ζ Can.mi	f	44	59	ſ	0	55	1	eF. pL. iF.
258	—	10 (ι) Virgin	p	5	2	ſ	0	7	2	vF. S. E.
	1785									
259	Jan. 6	70 Ceti	p	10	34	ſ	0	38	1	eF. eS. iF.
260	—	– –	p	7	10	n	0	4	1	eF. vS. ſtellar.
261	—	75 Ceti	p	3	46	ſ	0	6	1	vF. cL.
262	—	94 Ceti	p	1	16	ſ	1	15	1	eF. ver. 240 with difficulty.
263	—	24 Eridani	p	3	22	ſ	0	11	1	eF. ſtellar. or lE. almoſt ver. 240.
264	—	28 (A) Hydr	p	26	48	n	1	19	2	vF. vS. R. ver. 240.
265	10	45 (θ) Ceti	f	32	28	ſ	0	46	1	eF. ſtellar. ver. 240.
266	—	– –	f	31	6	ſ	0	43	1	vF. lE, ver. 240.
267	Feb. 4	14 (ζ) Lepor	f	0	1	ſ	1	56	1	vF. pS. iE. bM.
268	6	11 (α) Lepor	p	27	51	ſ	0	31	1	eF. vS. ſtellar. ver. 240. eaſily.
269	—	19 Leporis	p	32	23	n	1	11	1	eF. vS. ſtellar. ver. 240. eaſily.
270	—	– –	p	20	0	n	1	28	1	vF. eS. ſtellar. ver 240 difficulty.
271	—	8 (3 ν) Can'	f	8	0	n	0	4	1	3 or 4 Sſt with neb. vF. ver. 240.
272	7	6 (3 b) Crater	p	58	39	n	1	21	1	vF. pS. iF. vlbM.
273	—	– –	p	55	43	n	0	39	1	eF. vS. iF.
274	—	31 Crateris	p	4	40	ſ	0	14	1	vF. pL. iF.
275	8	12 Hydræ	f	20	30	ſ	1	49	1	vF. vS. bM. ½′ ſ. Sſt.
276	—	38 (κ) Hyd	p	9	20	ſ	0	26	1	vF. vS. ſtellar. 240. the ſame.
277 278	—	39 (1 υ) Hyd	p	5	0	n	0	30	1	Two. 3 or 4′ diſt. The moſt n. vF. S. The ſ. vF. vS. Both ſtell.
279	—	8 (η) Corvi	p	31	26	n	0	16	1	eF. pL. better with 157 than 240.
280	—	– –	f	18	44	n	1	51	1	½′ p. II. 298. eF. eS. ſtell. 240. doubtful.
281	—	– –	f	20	38	n	0	46	1	vF. pS. r.
282	—	53 Virginis	f	7	12	n	1	12	1	vF. mE. ſf np. v narrow.
283	17	41 (ω) Bootis	p	27	54	n	0	27	1	vF. vS.
284	Mar. 5	25 (f) Virg	p	54	12	ſ	0	19	1	vF. S. iE. lbM.
285	—	88 Virginis	f	8	45	n	1	17	1	eF. vS.
286	—	99 (ι) Virg	p	9	22	n	0	31	1	vF. L. b towards n.
287	—	– –	p	7	58	ſ	0	7	1	vF. pS. iF.
288	6	15 (ι) Navis	f	11	16	ſ	1	7	1	vF. cL. er. ſome of the ſt. viſ.
289	10	6 (3 b) Crat	p	69	14	ſ	0	25	2	F. vS. large ſtellar. lbM.
290	—	2 (ι) Corvi	p	16	1	n	2	3	1	eF. pL. broadly E. nearly par.
291	11	75 Cancri	p	2	53	ſ	1	13	1	vF. pL. R. bM.
292	12	46 Cancri	p	11	46	ſ	1	14	2	vF. pL. R. lbM. r.
293	—	23 Leonis	p	17	46	ſ	0	22	1	eF. eS. ver. 240.
294	13	57 (2 ι) Canc	p	2	44	n	0	15	1	vF. vS. R. bM. large ſtellar.
295	—	72 (τ) Canc	f	5	47	n	0	24	1	vF. vS. R. nf. 2pBſt.
296	—	– –	f	8	42	n	1	17	1	vF. S. R. lbM.
297	—	15 (f) Leon	p	13	8	ſ	0	34	1	eF. eS. 240 left a doubt.
298	—	18 Leonis min	p	20	56	ſ	0	44	2	vF. vS. iR. lbM.

III.	1785	Stars.		M.	S.		D.M.	Ob.	Deſcription.
299	Mar. 13	13 Can. ven.	p	40	51	ſ	0 27	1	eF.
300	—	– –	p	40	19	ſ	1 28	1	The moſt f of 3. vF. II. 322. 323.
301	—	– –	p	28	58	ſ	1 41	1	vF. vS R.
302	—	– –	p	27	40	ſ	1 2	1	eF. vS.
303	—	– –	f	5	43	ſ	1 43	1	eF. vS. ver. 240.
304	—	– –	f	6	26	ſ	1 47	1	eF. vS. ver. 240.
305	—	– –	f	11	0	ſ	1 9	1	vF. vS. lE.
306 307	—	– –	f	16	12	n	0 6	1	Two. The p. vF. vS. The f. 7 or 8′ nf the firſt. vF. vS.
308	—	– –	f	17	29	n	0 13	1	vF. S.
309	—	– –	f	18	31	n	0 34	1	eF. vS.
310	—	49 (δ) Bootis	p	43	12	ſ	1 32	1	vF. vS. iF.
311	16	11 Urſæ min	p	24	18	n	1 18	1	vF. S. iR. between 2 pSſt.
312	—	– –	p	19	9	n	2 6	1	eF. vS. lE. 2 vSſt in it.
313	—	13 (γ) Ur. mi	f	27	8	n	0 12	1	vF. vS. lE.
314	—	– –	f	49	18	n	0 24	1	eF. vS. lE. er.
315	Apr. 3	27 Urſæ	f	3	42	n	0 46	1	eF. vS. ver. 240.
316	—	– –	f	51	42	n	1 43	1	eF. pS mE. r.
317	—	– –	f	65	18	n	1 19	1	vF. vS.
318	—	– –	f	69	10	n	0 20	1	vF. pL. r.
319	—	7 (β) Urſ. mi	p	32	2	n	2 26	1	eF. not verified.
320	6	72 Leonis	f	26	8	n	1 44	2	vF. vS. ſtellar.
321	—	4 Comæ	p	22	54	n	0 15	1	vF. pS.
322	—	– –	p	19	8	n	0 18	1	vF. ſtellar.
323 324	—	– –	p	14 ..	43 ..	ſ	0 40	1	Two. The ſp. vF. lE. The nf. eF. 5 or 6′ diſt.
325	—	– –	p	13	46	ſ	0 45	1	eF. vS.
326	—	– –	p	5	47	ſ	0 17	1	eF. vS. ver. 240. diſcovered in gaging.
327	—	– –	p	1	45	n	0 33	1	vF. pS.
328	—	31 Comæ	p	6	28	ſ	0 25	2	F. S.
329	—	21 (*g*) Comæ	f	14	45	n	2 26	2	vF. S.
330	10	36 (ζ) Leonis	f	4	54	n	0 29	1	vF. pS. vlbM. iR.
331	—	41 Leonis mi	p	12	16	n	1 36	1	vF. vS. vlbM.
332	—	54 Leonis	f	2	46	ſ	0 34	1	vF. 1′ n. Sſt.
333	—	72 Leonis	f	2	48	n	0 18	1	vF. vS. ver. 240.
334	—	– –	f	3	17	n	0 23	1	vF. S.
335 336	—	– –	f	7	12	n	1 12	1	Two. 2 or 3′ diſtant. Both vF. vS. the moſt ſ. fainteſt.
337	—	– –	f	9	34	n	0 52	1	vF. S.
338	—	– –	f	25	56	ſ	0 38	1	vF. vS. 240. the ſame.
339	—	– –	f	26	30	n	1 44	1	vF. vS. 240. the ſame.
340	—	– –	f	28	36	ſ	0 19	1	vF. vS. pL. two ſtellar. ſuſpected near it.

III.

III.	1785	Stars.		M.	S.		D.	M.	Ob.	Deſcription.
341	Apr. 10	7 (*b*) Comæ	p	26	41	n	0	56	1	vF. vS. ver. 240. eaſily.
342	—	— —	p	22	55	ſ	0	32	1	vF. vS. lE.
343	—	— —	p	20	7	ſ	0	4	1	vF. vS. 240. the ſame.
344 345	—	— —	p	18	31	ſ	0	43	1	Two. 5 or 6′ diſtant. Both eF. vS. ver 240.
346	—	40 Comæ	f	1	38	n	2	8	1	eF. pL. lE. ver. 240.
347	—	12 (*d*) Bootis	f	7	40	ſ	1	17	1	vF. lE S.
348	11	23 Leonis min	f	3	12	ſ	1	38	1	eF. lE. a little doubtful.
349	—	39 Leonis min	p	9	28	n	1	18	1	eF. 240 ſhewed a few Sſt. with neb. but doubtf.
350	—	44 Leonis min	f	17	36	n	0	35	1	vF. S.
351 352	—	— —	f	20	58	n	0	51	1	Two. Both vF. vS. the moſt ſ. is the fainteſt.
353	—	— —	f	53	4	n	0	26	1	eF. 240 left it doubtful.
354	—	14 (*b*) Comæ	p	28	29	n	0	43	1	vF. vS. diſcovered in gaging.
355	—	— —	p	21	41	ſ	0	16	1	vF. S. pmE.
356 357	—	— —	p	17	40	n	1	55	1	Two of 3. the place is that of II. 371. Both vF. mE. A 4th ſuſpected.
358 359 360	—	— —	p	14	24	n	1	55	1	Three of a quartile. The place is that of II. 372. All vF. vS. and all within 3′.
361	—	— —	p	0	40	n	0	18	1	vF. vL.
362	—	15 (*c*) Comæ	f	3	2	ſ	1	3	1	eF. cL. 4 or 5′ l. 2′ b.
363	—	41 Comæ	p	6	16	n	0	23	1	vF.
364	—	— —	p	5	24	n	0	25	1	vF.
365	—	— —	f	1	8	n	0	41	1	vF.
366	—	— —	f	2	26	n	1	18	1	vF. pS.
367	—	43 Comæ	f	1	24	ſ	0	2	1	vF. pL.
368	—	— —	f	11	2	ſ	0	53	1	vF. mE. 1½′ l. r. diſcov. gaging.
369	—	— —	f	25	41	ſ	0	29	1	eF. vS. 240 left a little doubt.
370	—	— —	f	28	8	n	0	31	1	vF. S. mE. nearly mer.
371	—	14 (ι) Coron	p	13	52	ſ	1	8	1	vF. S. R. ver. 240 eaſily.
372	13	93 Leonis	p	1	25	n	0	25	1	vF. cL. moon-light.
373	14	11 Libræ	f	1	18	ſ	0	12	1	vF. juſt n. Sſt.
374	—	11 Serpentis	p	12	8	ſ	1	18	1	eF. pL. r.
375	25	93 Leonis	p	7	28	n	0	7	2	vF. vS. r.
376	26	— —	p	5	57	n	0	5	2	eF. vS.

Fourth claſs. Planetary nebulæ.

Stars with burs, with milky chevelure, with ſhort rays, remarkable ſhapes, &c.

IV.	1782	Stars.		M.	S.		D.M.	Ob.	Deſcription.
1	Sept. 7	13(ν)Aquarii	p	5	24	n	0 2	11	vB. nearly R. planetary not well defined diſk.
2	1783 Dec. 26	13 Monocer	f	6	4	n	1 27	4	eB, fan-ſhaped. about 2′ l. from the center. Fig. 7.
3	1784 Jan. 16	15 Monocer	p	8	18	n	0 15	4	pB. m. like a ſt. with an electrical bruſh. Fig 8.
4	Feb. 22	69 Leonis	f	10	3	f	1 3	2	eF. S. like an ſt. with a vF. bruſh ſp. 240 ſhews the ſt.
5	—	29 (γ) Virg	p	9	0	n	1 33	2	A pBſt. with a m. ray ſ. par. 15 or 20′ l. Fig. 6.
6	23	59 (*c*) Leonis	p	9	0	f	0 18	1	F. L. C. A central B. point with eF. m. chev.
7	Mar. 14	51 (*m*) Leon	f	17	0	f	0 39	2	F. pL. m. between 2 Bſt. like an electrical bruſh to the moſt n. but is not connected. R.
8 9	15	34 Virginis	p	10	12	f	0 51	2	A double Nebula. The che. run into each other. cloſe. not vF.
10	21	51 (*m*) Leo	p	21	15	f	1 48	1	A pcſt. with a vF. bruſh nf. with 240 2 vSſt. viſible in it, but not connected.
11	May 21	51 (*c*) Ophiu	p	1	42	n	0 14	2	pB. R. p. well defined planetary diſk. 30 or 40″ d.
12	24	3 (*p*) Sagitt	f	22	0	n	1 47	1	F. L. iR. inclining to m. 3 or 4′ d. like a bruſh to a np. ſt. but probably unconnected.
13	July 17	39 (*b*) Cygni 21 Vulpecu	p f	8 2	6 6	f n	1 35 1 51	2	pF. exactly R. of equal light. the edges p. well def. 1′d. See note.
14	21	27(*d*)Aquilæ	p	6	6	f	1 45	2	vF. of equal light. r. 1′ d. in the midſt of numberleſs ſt. of the milky way.
15	Sept. 8	21 (α) Andr	f	2	6	f	1 21	1	A Fſt. with S. chev. and 2 burs.
16	16	16(η)Sagittæ	f	17	12	n	0 1	2	pB. perfectly R. pretty well defined. ¼′ d. r.
17	20	81 Ceti	f	36	30	n	0 36	1	A Sſt. with a vF. nebulous bruſh 1½ or 2′ l. diſcovered with 240.

IV.

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IV.	1784	Stars.		M.	S.		D.	M.	Ob.	Deſcription.
18	Oct. 6	14 Androm.	p	6	11	n	3	16	4	B. R. a planetary p. well defined diſk. 15″ diaʳ with a 7 feet reflector.
19	16	5 Monoc.	p	7	6	f	0	10	1	A ſt. of the 9 magnitude, with m. chev. i elliptical.
20	—	- -	p	3	42	n	0	3	1	A ſt. of the 11 or 12 mag. affected like the foregoing, but vF.
21	Nov. 20	12 Leporis	p	8	48	n	0	24	1	vS. ſtellar. vBN. and vF. chev. not quite central.
22	Dec. 9	7 (ξ) Navis	f	3	10	f	1	28	2	L. pB. R. er. 6 or 7′ d. a faint red colour viſible. A ſt. 8 mag. not far from the center, but not connected. 2d ob. 9 or 10′ d.
	1785									
23	Jan. 6	75 Ceti	p	4	40	f	0	6	1	cB. a vBN. with a chev. of 3 or 4′ d.
24	—	50 (ζ) Orio	f	0	57	f	0	17	1	A Bſt. with m. chev. 5′ l. 4′ b.
25	31	19 Navis	p	67	0	n	1	15	1	A pcſt. with vF. and vS. m. chev. iF.
26	Feb. 1	34 (γ) Erid	f	16	16	n	0	49	2	vB. perfectly R. or vl. elliptical. planetary but ill defined diſk. 2d obſ. r. on the borders, and is probably a very compreſſed cluſter of ſtars at an immenſe diſtance.
27	7	6 (3*b*) Crater	p	28	39	n	1	25	2	Beautiful, brilliant, planetary diſk ill defined, but uniformly B. the light of the colour of Jupiter. 40″ d. 2d obſ. near 1′ d. by eſtimation.
28	—	31 Crateris	f	1	0	n	0	47	1	pB. L. opening with a branch, or two nebulæ very faintly joined. The ſ. is ſmalleſt.
29	8	4 (ν) Crateris	f	3	36	n	0	16	1	A Sſt. with an eF. bruſh p. perceived in gaging. ver. 240.

Fifth claſs. Very large nebulæ.

V.	1783	Stars.		M.	S.		D.	M.	Ob.	Deſcription.
1	Oct. 30	18 (ι) Piſ.auſt.	f	128	17	n	1	39	6	cB. mE. ſp nf. mbM. Above 50′ l. and 7 or 8′ b. C. H. See note.
	1784									
2	Jan. 24	10 (*r*) Virgin	f	24	46	n	0	17	4	cB. mE. np ſf. mbM. er. 9 or 10′ l with a branch towards the np.

V.	1784	Stars.		M.	S.		D.	M.	Ob.	Deſcription.
3	Jan. 24	75 Leonis	f	104	0	ſ	0	24	1	eF. vL. er. R. 7 or 8′ d.
4	Feb. 23	7 (*b*) Virgin	f	8	15	ſ	0	45	2	vF. R. 5 or 6′ d.
5	Mar. 14	11 Comæ	f	0	45	n	0	32	1	L. E. r. 6 or 7′ l.
6	21	4 (τ) Bootis	p	0	45	ſ	1	6	1	vL. eF. r.
7	Apr. 8	52 (K) Leonis	p	3	0	n	0	41	1	vL. F. r. almoſt R.
8	—	73 (*n*) Leonis	f	4	34	n	0	18	3	B. E. almoſt par. but l. np ſſ. near 15′ l.
9	May 22	51 (*e*) Ophiu	f	32	48	ſ	0	40	1	L. E. broad. m. F.
10 11 12	July 12	5 (*i*) Sagitti	f	2	42	n	0	49	1	Three nebulæ, faintly joined, form a triangle. In the middle is a double ſt. vF. and of great extent.
13	—	– –	f	4	54	n	0	39	1	Extenſive m neb. divided into 2 parts. the moſt n. above 15′. The moſt ſ. followed by ſtars.
14	Sept. 5	52 (*k*) Cygni	f	11	24	n	0	44	2	Branching nebuloſity, extending in R.A. near 1½ deg. and in P.D. 52′. The f. part divides into ſeveral ſtreams uniting again towards the ſ.
15	7	– –	f	0	0	n	0	0	3	Extended; paſſes thro' *k* Cygni. By the Newtonian view above 1 degree l. By the *Front-view* near 2 deg. l. See note.
16	11	28 Androm.	p	11	12	n	0	17	1	eF. 5 or 6′ d.
17	—	2 (α) Triang	p	18	48	n	0	55	2	m. nebuloſity. not leſs than ½ deg. broad. perhaps ¾ degree long, but not determined.
18	Oct. 5	35 (ν) Andr	p	9	11	n	0	37	4	vB. mE. 30′ l. 12. b. C. H.
19	6	26 (β) Perſei	p	45	11	n	1	16	3	cB. mE. above 15′ l. 3′ b. a black diviſion 3 or 4′ l. M.
20	20	7 (*b*) Ceti	f	33	9	ſ	1	48	1	A ſtreak of light, nearly mer. 26′ l. 3 or 4′ b. pB.
	1785									
21	Jan. 31	18 (μ) Canis	f	22	18	n	1	2	2	A broad E nebuloſity. forms a parallelogram with a ray ſouthwards; the parall. 8′ l. 6′ b. vF.
22	Feb. 7	61 Virginis	f	10	59	n	0	17	1	mE. ſſ np. 5 or 6′ l. pF.
23	Apr. 3	27 Urſæ	f	13	18	n	0	0	1	L. F. 1E. r. 6 or 7′ l. 5 or 6′ b.
24	6	21 (*g*) Comæ	f	5	20	n	1	25	1	A lucid ray 20′ l. or more. 3 or 4′ b. np ſſ. vBM. a beautiful appearance.

Sixth claſs. Very compreſſed and rich cluſters of ſtars.

Additional abbreviations	Cl. Cluſter.	com. compreſſed.
	ſc. ſcattered.	co. coarſely.

VI.	1783	Stars.		M.	S.		D.	M.	Ob.	Deſcription.
1	Nov. 19	63 (ρ) Gemi	f	11	0	n	0	12	3	A beautiful Cl. of many L. and com. S. ſt. about 12′ d.
2	Dec. 30	18 (ν) Gemi	f	27	10	ſ	2	9	3	A v. com. Cl. of eSſt. iF. 5 or or 6′ d.
3	1784 Jan. 24	12 Monocer	f	11	30	ſ	0	18	1	A Cl. of v. com. and eS. ſt. E.
4	—	4 Sextantis	f	5	30	ſ	0	5	1	A Cl. of v. com. S. ſt.
5	Feb. 11	31 (2ξ) Gem	p	31	0	ſ	0	15	1	A Cl. of v. com. S. 7 or 8′ d.
6	Mar. 8	67 Gemin	p	18	0	ſ	1	57	1	A Cl. of ſt. of various ſizes pm. com. M. p. rich.
7	14	42 Comæ	f	8	30	n	0	8	1	An eF. Cl. of eS. ſt. with r neb. 8 or 10′ d. ver. 240. beyond doubt.
8	Apr. 25	26 (χ) Virg	f	23	44	ſ	0	6	1	A v. com Cl. of ſt. 8 or 9′ d. e rich. iR. or lE.
9	May 17	11 Bootis	f	4	18	n	1	7	1	A Cl. of eS. and com. ſt. 6 or 7′ d. many of the ſt. viſible, the reſt ſo S. as to appear nebulous.
10	22	21 (α) Scorp	p	1	48	n	0	24	1	A v. com. and cL. Cl. of the ſmalleſt ſtars imaginable. all of a duſky red colour. the next ſtep to an er. neb.
11	—	39 Ophiuchi	p	13	24	ſ	0	26	1	A fine miniature of the 19 nebula of the Connoiſſ. des Temps (which is a Cl. of v. com. ſt. much accumulated M. 4 or 5′ d. all the ſt. red.) 2 or 2′½ d. the ſt. F. red.
12	24	43 Ophiuchi	p	12	42	n	1	36	1	Another miniature Cl. like the preceding, but rather coarſer.
13	June 24	10 (γ) Sagitt	p	14	48	n	0	18	1	A Cl. of S. and p. com ſt. of ſeveral mag. 5 or 6′ d. not v. rich.
14	July 11	9 Vulpec	p	4	0	n	0	33	1	A Cl. of eS. and v. com. ſt. a parallelogram of 4′ l. 2′ b. mer.
15	July 12	34 (σ) Sagitt	p	6	54	n	0	27	1	A ſuſpected Cl. of vFſt. of conſiſiderable extent. not ver.
16	Aug. 18	12(γ) Sagittæ	p	4	18	ſ	1	32	1	A vS. Cl of com. ſt.
17	Nov. 16	42 (1ω) Gem	p	54	53	ſ	0	29	2	A v. rich Cl. of v. com. and eSſt. 4 or 5′ d. A miniature of the 35 Cl. of the Conn. des T. which it precedes 1′ 18″ and is 2′ n.
18	1785 Mar. 4	11 Monocer	f	27	15	ſ	0	2	4	A v. com. and rich Cl. of vSſt. iF. 8 or 9′ d.
19	10	24 (1ſt) Libr	f	5	0	ſ	1	16	1	A beautiful L. Cl. of the moſt minute and moſt com. ſt. of different ſizes. 6 or 7′ d. iR. F. red colour.

VII.

Seventh claſs. Pretty much compreſſed cluſters of large or ſmall ſtars.

VII.	1784	Stars.		M.	S.		D.	M.	Ob.	Deſcription.
1	Jan. 18	90 (1 *c*) Tauri	f	11	0	ſ	1	30	2	A Cl. of L. ſcat. ſt. 10 or 12′ in extent, with a vacancy M.
2	24	8 Monocer	f	8	17	n	0	23	3	A beautiful Cl. of ſc. ſt. chiefly of 2 ſorts. the firſt L. the ſecond arranged in winding lines. contains the 12th Monoc.
3	Feb. 8	3 Leporis	p	72	30	ſ	0	30	1	A S. Cl. of com. ſt. ſome pL.
4	19	15 (2 *y*) Orio	f	3	6	n	1	10	2	A Cl. of pL. and p. com. ſt. c. rich. 20 or 25′ d. iR.
5	23	13 Monocer	p	3	15	ſ	0	28	1	A Cl. of com. ſt. of various mag. p. rich in Sſt. not R.
6	Mar. 16	50 Gemino	f	3	55	ſ	2	9	1	A p. rich and com. Cl. of ſt.
7	May 24	3 (*p*) Sagitt^{i}	f	15	54	ſ	0	8	1	A c. rich, but p. co. ſc. Cl. of ſt. l. more com. M.
8	July 17	41 (*i*) Cygni	f	5	42	ſ	2	1	5	A v. rich Cl. of pS. ſc. ſt. moſt of the ſame ſize. 20′ d.
9	19	12 Velpecu	p	0	5	n	0	30	2	A L. Cl. of p. com. ſt. moſt of one ſize.
10	Nov. 20	7 (ξ) Navis	f	5	56	n	0	40	3	A vL. Cl. of ſc. ſt. c. rich and com. more than 15′ d.
	1785									
11	Jan. 31	19 Navis	p	0	40	n	0	5	1	A c. rich Cl. of co. ſc. ſt. above 20′ d.
12	Feb. 4	6 Navis	p	31	59	n	1	25	4	A beautiful Cl. of p. com. ſt. near ½ deg. d. C. H.
13	6	2 (β) Canis	p	7	10	ſ	0	44	1	A Cl. of ſc. Sſt. not v. rich above 15′ d.
14	8	18 (μ) Canis	f	3	17	n	0	20	1	A Cl. of co. ſc. ſt. 20′ d.
15	Mar. 6	26 Canis	f	1	22	n	1	52	1	A S. Cl. of p. com. ſt. not v. rich.
16	—	– –	f	1	56	n	0	16	1	A Cl. of fc. ſt. c. rich. 20′ d.
17	—	– –	f	6	26	n	1	1	2	A v. beautiful Cl. of pL. ſt. v. rich. contains the 30 Canis.

Eighth claſs. Coarſely ſcattered cluſters of ſtars.

VIII.	1783	Stars.		M.	S.		D.	M.	Ob.	Deſcription.
1	Dec. 3	14 Navis	p	4	0	n	0	40	2	A Cl. of co. ſc. ſt. The place is that of the moſt com. part which is not M.
2	26	58 (α) Orion	p	8	28	n	1	16	2	A S. Cl. of vS. ſc. ſt.
3	—	13 Monocer	f	1	30	n	1	2	2	An E. Cl. of L. ſc. ſt.

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VIII	1784	Stars.		M.	S.		D.	M.	Ob.	Deſcription.
4	Jan. 16	112 (β) Tauri	p	0	51		0	38	3	A Cl. of co. and i. ſc. pLſt.
5	18	15 Monocer	p	0	0	n	0	0	3	Double and attended by more than 30 cLſt.
6	24	8 Monocer	p	14	20	n	0	4	2	A Cl. of co. ſc. ſt. not rich.
7	Feb. 10	4 Orionis	p	4	0	ſ	1	7	1	A Cl. of L. and S. ſc. ſt. not rich.
8	15	97 (*i*) Tauri	p	5	28	n	0	13	2	A Cl. of cL. v. co. ſc. ſt. perhaps a projecting point of the m way.
9	19	24 (γ) Gemi	p	8	15	n	0	15	1	A Cl. of vm. ſc. ſt. of various magnit. near ½ deg. not rioh.
10	Mar. 15	50 (2 A) Canc	f	3	0	ſ	0	44	1	A Cl. of v. co. ſc. ſt. not rich.
11	16	50 Gemini	f	15	55	ſ	2	19	1	A Cl. of ſc. ſt.
12	June 16	1 (*m*) Aquilæ	f	1	42	n	0	2	1	A Cl. of v. co. ſc. ſt.
13	—	20 Aquilæ	p	12	48	ſ	0	56	1	A Cl. of co. ſc. ſt. not rich.
14	18	43 (*d*) Sagitt^{l}	p	44	48	n	1	54	1	A Cl. of ſc. pLſt.
15	July 15	63 Sagittarii	p	103	36	n	2	1	1	A Cl. of co. ſc. ſt.
16	17	12 (φ) Cygni	f	13	6	ſ	0	44	1	A Cl. of not v. com. ſt. cloſeſt M. It may be called (if the expreſſion be allowed) a forming Cl. or one that ſeems to be gathering.
17	18	33 Vulpec	p	24	18	n	0	4	1	A Cl. of many L. ſc. ſt.
18	Sept. 4	61 (φ) Aquilæ	p	2	54	n	0	18	1	A S. forming Cl. of ſt.
19	—	– –	p	0	42	n	0	40	1	A Cl. of co. ſc. L. ſt. not rich.
20	9	18 Vulpec	f	1	0	ſ	0	27	1	A Cl. of co. ſc. ſt. not rich.
21	10	6 Vulpec	p	2	27	n	0	29	2	A Cl. of cL. co. ſc. ſt.
22	—	18 Vulpec	f	1	12	ſ	0	12	1	A Cl. of co. ſc. ſt.
23	Oct. 15	12 (γ) Delph	p	5	18	n	0	33	1	A Cl. of co. ſc. ſt.
24	—	67 (ν) Orion	f	1	0	ſ	0	46	1	A SCl. of pL. white ſt.
25	16	10 Monocer	f	0	0	ſ	0	0	1	The 10 Monoc. ſurrounded by by many Bſt.
26	Nov. 16	1 (H) Gem	p	2	16	n	0	3	1	A Cl. of ſt. of various magnit. not v. rich. 6 or 7′ d.
27	20	11 (*e*) Navis	p	36	41	n	0	46	1	A S. Cl. of ſc. ſt. not rich, nor v. com.
28	Dec. 5	54 (1λ) Orion	p	11	53	ſ	0	15	1	A Cl. of pL. ſc. ſt. not rich.
29	9	101 (4*b*) Aqu	f	32	30	n	0	11	1	A Cl. of a few co. ſc. L. ſt.
30	—	25 (δ) Canis	f	57	10	ſ	1	15	1	A vL. Cl. of many co. ſc. L. ſt.
	1785									
31	Jan. 6	19 Monocer	p	15	36	n	1	3	1	A L. Cl. of ſc. ſt. not v. rich.
32	10	26 Monocer	p	34	32	ſ	0	41	1	A Cl. of co. ſc. ſt. of many magn. p. rich. above 15′ d.
33	—	– –	p	32	50	ſ	1	15	1	A Cl. of ſc. L. ſt.
34	—	– –	p	26	36	ſ	0	52	1	An extenſive Cl. of ſc. ſt.
35	31	2 Navis	p	21	23	n	1	21	3	A Cl. of pL. ſc. ſt. p. rich. about 20′ l. crooked fig.

VIII.	1785	Stars.		M.	S.		D.	M.	Ob.	Deſcription.
36	Jan. 31	19 Navis	p	43	20	n	1	0	1	A forming Cl. of co. ſc. ſt. 20 or 30′ dia.
37	Feb. 4	6 Navis	p	16	47	n	1	43	2	A S. Cl of p. com. ſt. of various ſizes. not v. rich.
38	—	2 Navis	p	8	55	n	0	10	2	A Cl. of p. com. L. and S. ſt. R. above 15′ d.
39	Mar. 4	11 Monocer	f	23	36	n	0	3	3	An extenſive Cl. of ſc. ſt. of various ſizes.
40	11	47 Geminor	p	4	2	n	0	18	1	Cluſtering L. ſc. ſt. many of equal ſize.

Notes to ſome Nebulæ and Cluſters of Stars.

I. 7. This remarkable appearance being no longer in the place it has been obſerved, we muſt look upon it as a very conſiderable teleſcopic comet. It was viſible in the finder and reſembled one of the bright nebulæ of the Connoiſſance des Temps ſo much, that I took it for one of them till I came to ſettle its place; but this not being done till a month or two after the obſervation, the opportunity of purſuing and inveſtigating its track was loſt.

I. 13.. The figures referred to, in the deſcription of this and ſome other nebulæ, may be found in the Philoſophical Tranſactions, vol. LXXIV. tab. XVII. p. 450.

I. 28. The numbers annexed to ſome of the nebulæ refer to the claſs and number of the preceding Catalogue: thus, II. 41. denotes that the 41ſt in the ſecond claſs is the third nebula, following the two here deſcribed.

I. 28. Near the 84. and 86. neb. of the Connoiſſance des Temps.

II. 6. This has probably been a teleſcopic comet, as I have not been able to find it again, notwithſtanding the aſſiſtance of a drawing which repreſents the teleſcopic ſtars in its neighbourhood.

II. 55. The preceding is the 85 of the Connoiſſance des Temps.

II. 84 6 or 8′ following the 100 of the Connoiſſance des Temps.

II. 118. Juſt following the 88. of the Connoiſſance des Temps.

II. 123. 124. The third is the 87th of the Connoiſſance des Temps.

III. 44. The following is the 60th of the Connoiſſance des Temps.

IV. 13. Before the value of the degree was more ſtrictly aſcertained, the two obſervations were thus:

21 Velpeculæ	f	2′ 6″	n	1° 51′
39 (*b*) Cygni	p	8 6	ſ	1 35

which, if there be no error in the place of the ſtars in FLAMSTEED's Catalogue, differ about 14′ in polar diſtance, for which reaſon in the ſecond Paper on the Conſtruction of the Heavens this nebula was put down twice, whereas it now appears, that both obſervations belong to the ſame.

V. 1. This nebula was diſcovered Sept. 23, 1783, by my ſiſter CAROLINE HERSCHEL, with an excellent ſmall Newtonian *Sweeper* of 27 inches focal length, and a power of 30. I have therefore marked it with the initial letters, C. H. of her name. See alſo V. 19. diſcovered Aug. 27, 1783, and VII. 13. diſcovered Feb. 26, 1783.

V. The *Front-view* is a method of uſing the reflecting teleſcope different from the Newtonian, Gregorian, and Caſſagrain forms. It conſiſts in looking with the eye glaſs, placed a little out of the axis, directly in at the front, without the interpoſition of a ſmall ſpeculum; and has the capital advantage of giving us almoſt double the light of the former conſtructions. In the year 1776 I tried it for the firſt time with a 10 feet reflector, and in 1784 again with a 20 feet one; but the ſucceſs not immediately anſwering my expectations, it was too haſtily laid aſide. By a more careful repetition of the ſame experiment I find now, that ſeveral other conſiderable advantages, added to the brilliant light before mentioned, make it ſo valuable a conſtruction that a judicious obſerver may avail himſelf of it at leaſt in all caſes where light is more particularly wanted; and from the experience of 30 ſweeps, which I have already made with it, I may venture to announce it to be a very convenient and pleaſant, as well as uſeful, way of obſerving. With regard to the poſition of objects, it differs from other conſtructions, by inverting the north and ſouth, but not the preceding and following.

Errata of the Catalogue.

The following nebulæ ſhould ſtand thus.

I. 54.	35 (ν) Andr.	f	18 36	ſ	1 26	1	pB. S. R. vgbM.
II. 1.	41 Aquarii	p	11 45	n	0 17	2	F. cL. mE. bM. cr.
II. 239.	In the deſcription read						The 2d of two.

XXVIII. *Inveſtigation of the Cauſe of that Indiſtinctneſs of Viſion which has been aſcribed to the ſmallneſs of the Optic Pencil.* *By* William Herſchel, *LL.D. F.R.S.*

Read June 22, 1786.

SOON after my firſt eſſays of uſing high powers with the Newtonian teleſcope, I began to doubt whether an opinion which has been entertained by ſeveral eminent authors, "that "viſion will grow indiſtinct, when the optic pencils are leſs "than the 40th or 50th part of an inch," would hold good in all caſes. To judge according to ſo rigid a criterion, I perceived that I was not intitled to ſee diſtinctly with a power much more than about 320, in a 7-feet teleſcope which bore an aperture of 6,4 inches; whereas in many experiments on double ſtars I found myſelf very well pleaſed with magnifiers that far exceeded ſuch narrow limits. This induced me, as it were, by way of apology to myſelf, for ſeeing well where I ought to have ſeen leſs diſtinctly, to make a few experiments on the ſubject of the diameter of optic pencils. It occurred to me, that an opinion which limits them to any given ſize cannot be ſupported by theory, which does not determine on ſubjects of this nature, but muſt be decided, like many other phyſical queſtions relating to matters of fact, by careful experiments made upon the ſubject. The way, therefore, to come at truth, in a caſe which ſeemed to me of conſiderable importance, lay ſtill open to me, as it had done to former obſervers; and I thought myſelf authoriſed, according to a Carteſian maxim (*Dubia etiam pro falſis habenda*), to ſuppoſe, for a while, the ſize of optic pencils, requiſite for diſtinct viſion, intirely undecided.

The firſt opportunity I had of making the propoſed experiments was in the year 1778, and the reſult of them proved ſo deciſive that I have never ſince reſumed the ſubject; and had it not been for a late converſation with ſome of my highly eſteemed and learned friends, I might probably have left the papers, on which theſe experiments were recorded, among the reſt of thoſe that are laid aſide when they have afforded me the information I want. But a doubt ſeeming ſtill to be entertained on the ſubject of the ſmallneſs of the optic pencils, it may now be proper for me to communicate theſe experiments, that it may appear how far the concluſions I have drawn from them are warranted by the facts on which I ſuppoſe them to reſt.

Experiments with the naked eye.

Exp. 1. Through a very thin plate of braſs I made a minute hole with the fine point of a needle; its magnified diameter, very accurately meaſured under a double microſcope, I found to be ,465 of an inch, while under the ſame apparatus a line of ,05 in length gave a magnified image of 3,545 inches. Hence I concluded, that the real diameter of the perforation was about the 152d part of an inch. Through this ſmall opening, held cloſe to the eye, I could very diſtinctly read any printed letters on which I made the trial. Proper allowance muſt be made for the very inconvenient ſituation of the eye, which by the unuſual cloſeneſs to the paper cannot be expected to ſee with its common facility. Beſides, the continual motion of the letters, which is required on account of the ſmallneſs of the field of view, muſt needs take up a conſiderable time.

Exp. 2. In ſome other pieces of braſs I made ſmaller holes; and among many, that were meaſured with the ſame accuracy as in the former experiment, I found one whoſe magnified diameter was ,29: hence the real diameter could not exceed the 244th part of an inch. Through this opening I could alſo read the ſame letters; but the difficulty of managing ſo as not to intercept all the incident light, as well as the very uneaſy ſituation of the eye, were ſufficient reaſons for not carrying the intended experiments any further under this form. Beſides, I ſhould hardly have allowed them to be fair, if, on a further contraction of the hole in the braſs plate, an indiſtinctneſs had come on; as we might well have ſuſpected at leaſt two other cauſes, beſides the ſmallneſs of the pencils, to contribute to ſuch an imperfection; *viz.* want of light, and a deflection of it on the contracted edges of the hole.

Microſcopic Experiments.

Exp. 3. I had now recourſe to a double microſcope, conſiſting, for ſimplicity's ſake, of only two lenſes. The focal length of the eye-glaſs, carefully aſcertained by an object half a mile off, being ,9; the diſtance of the object-glaſs from the eye-glaſs 9,36; and the aperture of the object-glaſs ,0405. Hence we compute that the diameter of the optic pencil, when it entered the eye, could not exceed the 232d part of an inch; yet with this conſtruction I ſaw very diſtinctly every object I placed under the microſcope.

Exp. 4. I reduced the aperture of the object-glaſs to ,013; hence the pencil was found to be the 724th part of an inch; and yet I ſaw with this conſtruction very diſtinctly every object that was placed under the magnifier.

Exp. 5. I made a ſecond reduction of the aperture of the object-glaſs, ſo that now it was no more than ,0052; and therefore the optic pencil leſs than the 1800th part of an inch; and yet I could very well count the briſtles on the edge of the wing of a fly, and diſtinguiſh their length and thickneſs.

Exp. 6. Changing the conſtruction of the microſcope, I now reduced the pencils by an increaſe of power. Solar focus of the eye-glaſs ,52; diſtance between the object-glaſs and eye-glaſs 7,6; aperture the ſame as in the third experiment. This gave me a pencil of the 336th part of an inch, with which I ſaw very diſtinctly.

Exp. 7. Applying now the reduced aperture of the fourth experiment, I had a pencil of the 1139th part of an inch, with which I ſaw very well.

Exp. 8. I changed the eye lens for another of ,171 focal length; the object-glaſs and diſtance between the two lenſes remaining as in the two laſt experiments; aperture ,02. This gave a pencil of the 2173d part of an inch, with which I could count, or rather ſucceſſively ſee, the briſtles before-mentioned very well; the field, on account of the great power, not taking in more than two large and a ſmall one at a time.

Exp. 9. I was now convinced, that we may ſee diſtinctly with pencils incomparably leſs than the 40th or 50th part of an inch; and indeed ſo far from expecting any obſtruction to diſtinct viſion from the ſmallneſs of the pencils, it appeared to me now as if their ſize might in future be intirely left out of the account. With a view, however, of ſeeing what other cauſe might bring on that indiſtinctneſs which had been aſcribed to the ſmallneſs of the optic pencils, I continued theſe experiments with a variation in the apparatus, and uſed now an object lens of a different focal length; the aperture and other particulars being as in the 4th experiment. By this conſtruction, which gave me a pencil of the 724th part of an inch, I could ſee objects very well; but though they appeared diſtinctly, they were not ſo ſharp on the edges as one would wiſh to ſee them. This being compared with the 4th experiment, it appeared that, with equal pencils, unequal degrees of diſtinctneſs may take place; and a pretty ſtriking circumſtance, which ſerved to lead me in the following experiments, was, that the ſmalleſt power gave me the leaſt diſtinct image; notwithſtanding, from former trials, the goodneſs of the lenſes I employed could not be doubted.

Exp. 10. On an examination of circumſtances it occurred to me, as indeed I had already before ſurmiſed, that a certain proportion of aperture might be neceſſary to a given focal length of an object-lens or ſpeculum; and that a failure in this point might probably bring on that indiſtinctneſs which had been aſcribed to the ſmallneſs of the pencils. In order, therefore, to put this to a trial, I uſed now an object-lens of 1,25 focal length, with an aperture confined to ,01; the reſt of the apparatus being as in the 3d, 4th, and 5th experiments. The pencil in this caſe was about the 1000dth part of an inch; and though by a different conſtruction I had already ſeen very well with a pencil of not half that diameter, I found this to give me, as now I had reaſon to expect, a very indiſtinct picture, ſo much ſo indeed, that it could hardly be called a repreſentation of the object.

Exp. 11. Increaſing the aperture of the object-lens to ,0124, I had a pencil of the 758th part of an inch, but could ſee no better with it.

Exp. 12. Proceeding in the track now pointed out to me, I admitted an aperture of ,017, which gave a pencil of the 550th part of an inch, but could ſee not much better with it than before.

Exp. 13. On a farther increaſe of the aperture to ,0231, and a pencil of the 406th part of an inch, I ſaw a little better; but ſtill had not diſtinctneſs enough even to ſee the briſtles before-mentioned at all. Hence we may conclude, that, in ſuch conſtructions as the preſent one, the aperture of the object-glaſs muſt bear a conſiderable proportion to its focal length; ſince the 54th part (for ,0231 : 1,25 :: 1 : 54) is here not nearly ſufficient.

Exp. 14. To the ſame apparatus I applied a higher power, by an exchange of the eye-glaſs; but the indiſtinctneſs remained as before.

Exp. 15. Returning again to the former conſtruction, I admitted an aperture of about ,037; and having now a pencil of nearly the 250th part of an inch, I could but juſt perceive ſome of the large briſtles, which ſhews that even the 34th part (for ,037 : 1,25 :: 1 : 34) of the focal length is not a ſufficient aperture for object-lenſes that act under ſuch circumſtances as the preſent.

So far I have only related experiments that were made in the year 1778; and my opinion that the ſmallneſs of the optic pencils could be no objection to ſeeing well being thus ſupported by evident facts, I heſitated not, in a Paper on the Parallax of the Fixed Stars (Phil. Tranſ. vol. LXXII. p. 96.) to affirm, that we might ſee diſtinctly with pencils much ſmaller than the 40th or 50th part of an inch. It did not appear to be neceſſary, nor would the ſubject of that Paper permit me to enter into a detail of experiments; but having, in the courſe of my reading about that time, met with an account of ſome very ſmall globules made for microſcopic uſes, I contented myſelf with an inſtance of ſmall pencils taken from them. I ſhall, however, now proceed juſt to hint at a few inferences that may be drawn from theſe related experiments; as, upon a mature conſideration, we may find reaſon to believe they point out a cauſe of indiſtinctneſs of viſion hitherto never noticed by optical writers; and which, when properly inveſtigated, cannot but influence, and in ſome reſpects contribute to the improvement of, our theories in optics. For, admitting that every object-glaſs or ſpeculum, whoſe aperture bears leſs than a certain ratio to its focal length, will begin to give an indiſtinct picture, it will follow, that while former opticians have been endeavouring to diminiſh the aberrations ariſing from the ſpherical figure, and the different refrangibility of rays, by increaſing the focal length, they have been unaware of expoſing themſelves to the conſequences of the cauſe of indiſtinctneſs here pointed out. And till its influence ſhall be well aſcertained and brought to a proper theory, we muſt ſuſpect that ſuch tables as thoſe which are given in our beſt authors of optics, pointing out an aperture of leſs than 6 inches for a glaſs of 120 feet focal length (or a ratio of 1 to 240) muſt be far from having that degree of perfection which may yet be obtained. No wonder that teleſcopes, made according to theories or tables, where one of the cauſes of indiſtinctneſs is unſuſpected, and therefore left out of the account, can bear no ſmaller pencil than the 40th or 50th part of an inch! If then, on one hand, by increaſing our apertures we certainly run into great imperfections, we ought nevertheleſs alſo to conſider what dangers, on the other, we may incur by leſſening them too much.

As ſoon as convenient, I intend experimentally to purſue this ſubject, in order to obtain proper *data* for ſubmitting this cauſe of optical imperfection to theory; at preſent my engagement with the work of a 40-feet reflector will hardly permit ſo much leiſure; and till I ſhall have repeated, extended, and varied theſe experimental inveſtigations, I would wiſh them to be looked upon as mere hints that may afford matter for future diſquiſitions to the theoretical optician.

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